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CHANGES IN THE BOARD OF EDITORS

The Editor of the JOURNAL OF THE HISTORY OF MEDICINE AND ALLIED SCIENCES announces with deep regret the resignation of Max H. Fisch, Professor of Philosophy at the University of Illinois, from the Board of Editors on which he has served since the Journal was founded in January of 1946. During the first two years Professor Fisch also served as Editor of the section then called "Notes and Queries."

At the same time, the Editor takes pleasure in announcing that Dr. Fred B. Rogers, Associate Professor of Preventive Medicine at Temple University School of Medicine, Philadelphia, Pennsylvania has been appointed to the Board. Dr. Rogers is the author of a number of papers on the history of medicine, is Chairman of the Section on Medical History of the College of Physicians of Philadelphia, and is Editor of its *Transactions & Studies.*

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Obiter Dicta Bibliographica

F. J. COLE*

EARLY in 1957 I passed my 85th birthday, and my long career as a zoo-bibliophile is nearing its close. It started some 66 years ago when I became interested in animal structures. How and why I do not remember. I turned my bedroom into a miniature laboratory and commenced by dissecting earthworms, having been inspired to do so after reading Darwin's work on that despised Annelid. It was only later that I discovered Ray Lankester's three astonishing papers on the anatomy of the earthworm, written in 1864, when he was 17 years of age! At the same age a notice in *Nature* directed my attention to the classical paper on the development of the earthworm published in 1889 by that "quiet cultivated gentleman" Edmund B. Wilson. Having with some difficulty obtained a copy of this paper at the cost of many weeks' pocket money, it was disheartening to find that I could understand scarcely a single page of it. It was not the first memoir I had bought, but I have always regarded it as the first to arouse that bibliographical meticulosity which was to develop into a dominating influence throughout the rest of my long life. Wilson's masterly study left me in no doubt as to the supreme difficulty of the work, added to which were the shrewd demands on the draughtmanship of the author and the perfect skill of the lithographers who reproduced it. Here I must mention the greatest of all scientific lithographers, Messrs. Werner and Winter of Frankfurt a/M. A superb example of their work can be seen in Ashworth's paper on the giant nerve cells of *Halla*, published in the *Philosophical Transactions* in 1909. To produce those lovely and delicate effects of colour and shade, 13 impressions of each copy had to be registered and struck off, and the result astonishes and delights the artistic sense. No more shall we see such beautiful plates in a zoological treatise. The modern author is often no artist, and the cheap photographic blocks of line and stipple, however skilfully they may be drawn, can never challenge the superlative craftsmanship of Werner and Winter.

One of my earliest acquisitions became memorable for the following circumstance. I was then employed by the Professor of

* Henley-on Thames, England.

Natural History in Edinburgh as his private assistant at a salary of one pound a week, on which I had to live. The fashionable study at the time in the zoological autocracy was embryology, and when I saw in the window of Livingstone's bookshop a copy of Balfour's Comparative Embryology for 15 shillings, I determined to buy it. But how? With care I could save only 3 shillings a week, so that it would take five weeks to raise the full amount and in the meantime the book might be sold. However, it was still there when I entered the shop, and it was Mr. Livingstone himself who served me. I noticed that he greeted me with a keen glance as I approached the counter. I received the book, paid over the price, and had turned round to leave when he called me back. "Keep the book," he said, "as long as you like, and if you return it in good state I will refund the 15 shillings." That sympathetic and generous Scot recognized a serious but impecunious scholar when he saw him.

Perhaps the man who more than any other substantiated my interest in bibliography was G. B. Howes. He was the most learned zoologist I have ever met, and his own enthusiasm was inevitably communicated to all those who worked with him. Every morning he would arrive at my bench with an armful of books, the contents of which I was expected to assimilate the same day, himself never failing to point out how each author had strayed from the sublime path of accuracy and understanding. On visiting him at his home I was surprised to find that, for a man who loved and revered the literature of his subject, he had very few books of his own. Working as he did at South Kensington in the vicinity of large specialist libraries perhaps explained this anomaly. He told me that if, through illness, he had to miss a month's publications, the gap in his notes must remain, and he could only hope that that month's output was not important.

I often think that what I have done in assembling a valuable historical library of some 8,000 volumes can never be repeated under modern conditions, and this for two reasons. First, the books themselves are rapidly becoming absorbed into university and society libraries. Many countries such as Japan, and especially those which have suffered losses of libraries during the wars, are willing to pay anything for the early classics of science. Second, book prices have soared out of all reason, in fact so much so that only the wealthy can afford to purchase them. For example, a recent catalogue quotes Severino's *Antiperipatias*, 1659, at 75 pounds. My good copy, bound in contemporary vellum and a little foxed (as usual), cost 7 shillings. Also at 7 shillings I pur-

chased the same author's *Zootomia,* 1645, which was recently catalogued at 94 pounds. Two works by Rathke priced at 15 pounds and 10 pounds cost me ninepence and sixpence respectively. The first edition of George Stubbs' *Anatomy of the horse,* 1766, sells for 26 pounds, as against 6/6 when I bought mine. Von Baer's rare but not very important memoir on the development of fishes, published in 1835, is now priced at 18 pounds. My own copy, which bears the memorable signature of Alexander Goette, cost sixpence. On 6 December 1754, at the sale of the library of Dr. Richard Mead, a copy of the first edition of Harvey on the Circulation, 1628, sold for 4/6. The latest available copy, sold in 1953, realized 742 pounds, *in spite of the fact that the title-page was missing,* and but for this important defect the figure would probably have been 1,000 pounds. The noble work of Casserius on the vocal and auditory organs, 1601, 1600, which I bought for 2 pounds, now realizes 95 pounds. These are not exceptional cases, but only a selection from hundreds of others. Occasionally, however, inflationary prices mean nothing more than that angry rivals at an auction sale operate against each other and so build up the price. In one such case a book worth no more than 135 pounds actually realized 520 pounds.

Book prices may be artificially inflated as the result of a gamble in the antiquarian book fraternity. A curious instance of this in recent years has been the fallacious rarity of the first edition of Darwin's *Origin of species.* This famous work appeared on 24 November 1859 in a small edition of 1250 copies, all of which were sold on the day of publication. In spite of the fact that the first edition had never been a notably rare book, and that an excellent copy, such as my own, could have been purchased for 5 pounds, a London bookseller started the spiral by cataloguing a "very fine copy of this rare book" for £52.10.0. This *ballon d'essai* having found its mark, another "very fine copy of this rare book" appeared—this time attractively described in large type, *and occupying three inches of catalogue space.* Price 95 pounds. The 100-pound mark was soon passed at 118, and the top limit was reached with a copy briefly dismissed in only a single line of the catalogue, but priced at 130 pounds. The result was inevitable. Owners of the work, and they were not a few, crowded into the market, which was soon flooded. Prices came toppling down, and you could now acquire the 130-pound copy for 10 or less. Recently the book has started to climb again and the last copy I have recorded was priced at 21 pounds. It should be noted that the first

issue should include on page 184 the incredible suggestion of the black bear evolving into an aquatic species as monstrous as a whale, and also the misprint *speceies* on page 20, line 11.

Starting as I did in 1889, when the early literature was available and no one seemed to want it, a library of the classics of zoology could be built up by a young man of small means but scholarly outlook. At that time, particularly in Germany, there existed in every town of moderate size an enterprising bookseller who combed out the surrounding district for his stock and duly catalogued it. Before the first war, I could count on receiving every morning five or more book catalogues from various parts of Europe. The war killed all that, and it never came back, or if it did, catalogues were no longer circulated. The result was that during a considerable part of my life as a collector, books were plentiful and the cost within my means. My library, in fact, is on that account especially interesting, since it is a product of the fiscal revolution through which the antiquarian book trade has passed since 1914.

All book collectors have their ups and downs. I have been exceptionally fortunate, and my downs are few in number and have rarely disturbed a night's rest. And let me emphasize now that the bookseller is after all a human being. A distinguished friend, now deceased, once remarked to me that he could not understand why his book orders so often failed to produce the books. I could have told him that slackness in paying his bills had much to do with it. The bookseller naturally favours the customer who gives him no trouble over the bill. Pay your bills promptly, but do not encourage the bookseller to catalogue copies which are imperfect or in poor condition without describing their defects.

One of the failures which still rankles in my memory was the first edition of Markham's *Maister-Peece*, dated 1610. Of this evil and ridiculous work, which went through 21 editions and even survived for a hundred years the death of its author, only two copies had been recorded. One was listed in Bodley catalogues of 1674 and 1843. The other, described erroneously as "probably unique," was sold at Sotheby's in 1916 for £32 as lot 4707 of the famous Huth library. A third now appeared in the catalogue of an obscure London bookseller who, in ignorance of its value, priced it at 10s. I wired for it at once, but failed to lead the field, which must have been overwhelming.

Of all the booksellers with whom I have had business relations, the late Mr. John Grant of Edinburgh was held in high esteem by

all his clients. A friend of mine had occasion to telephone to him for a book appearing in one of his catalogues which he urgently needed. Mr. Grant himself took the call, but could only express regret that the book had just been sold. "Oh, damn" was the explosive response. There was a slight pause, and then the quiet and gentle voice of Mr. Grant stole upon the ear with "I beg your pardon, Sir." He once consulted me about an early medical work of 1554, the title-page of which bore the magic name of Vesalius. The book itself was of little value, but was the signature genuine? The price was £5. After a critical examination, I was convinced that the signature was that of Vesalius, which at once set a special value on the volume, but I decided to pass it on to a friend whose command of £5 notes exceeded my own. He kept it for several years and finally sent it to Hodgson's rooms to be auctioned. It fetched £120! And now this exemplar makes its last public appearance in the catalogue of the great library assembled by Dr. Erik Waller and bequeathed to the library of Uppsala University, where it appears as "the one genuine autograph of Vesalius known in a book." This depressing lapse on my own part reminds me of an occasion long ago when I was seated near Pachmann at one of his inspired interpretations of Chopin. I was quite familiar with the composition he was playing, during which he struck a wrong note. Without stopping, he turned round to where I was sitting and said "a mistake!"

However, ups are more interesting and important than downs, and here is a very singular one. George Poynder's bookshop in Broad Street, Reading, was a small quaint eighteenth century corner building which you entered by falling down three steps. I passed it every day when walking home and occasionally looked in to have a chat with Poynder and to discuss the fluctuations of the book market. On one such occasion I found on his desk an auction catalogue of the contents of a small mansion in Cornwall, the property of one of the lesser county squires. Only a very few books were included but amongst them, to my intense astonishment, was a copy of the first edition of Ruini's *Anatomy of the Horse,* Bologna, 1598. Now this is an exceedingly rare and important book, and there are probably not more than three or four copies of the first edition in England. I at once arranged that Poynder should attend the sale in person, and I saw him off at the station the day before the sale with the parting injunction, "Don't come back without it." We secured the copy for 25 shillings, and of course I had to pay his expenses and compensate him for the three

days he was absent from his business. How interesting the history of this copy would be, and what were its wanderings before it settled down in an obscure and lonely country house in Cornwall? And why should the auctioneer, seeing that only a handful of books was concerned, have sent a notice of the sale to a small, unknown provincial bookseller two hundred miles away? The Cornwall copy is a very good one and displays an excellent engraved bookplate by B. Wyon of Henry Champernowne and his wife whom I have not succeeded in tracing. The book now sells for £175.

Our local aristocrat in these parts owns a large and beautiful Elizabethan E-plan house situated in a deer park. For some reason, which naturally I am not in a position to disclose, he decided to sell off his library, which was housed in one of the wings of the E. I attended the sale, but the only book I wanted was a set of Leeuwenhoek's letters in five volumes, to get which, owing to misguided "lumping" on the part of the local auctioneer, I had to purchase 250 others. The lot was secured for £10, and amongst the 'lumpers' there were a few worth keeping and one very rare and important medical work only known to me through a timely critique by Professor Fulton. This I sold for £10, and so I got the Leeuwenhoeks and the others for nothing. However, as the Leeuwenhoeks were duplicates, I kept them for a short time and then sold them with a few other duplicates for £125. These Stonor volumes were specially interesting for the original and unobtrusive method of press-marking, the section-letter and shelf-figure being neatly stamped in gold on a calf label only 9 mm. square, which was pasted on to the bottom panel of the spine. Such attractive and ingenuous labels encourage the pleasant reflection that the books must at some time have been in charge of a librarian who loved them.

The *Acta Medica Hafniensia,* 1673-1680, is a very scarce and indispensable publication in five volumes by the Copenhagen biologists under the impelling influence of Thomas Bartholin, the son of Caspar. Apart from my own copy, which belonged to Clopton Havers of the Haversian canals, 1691, and bears his signature, I have records of only two other complete copies which were priced at £27.10.0 and £30. I had been looking out for a copy for many years when one broke the surface in the catalogue of a London bookseller, price 25s. An anxious rush to town secured it; but my satisfaction was dashed to find that all the plates were missing from volume 3. There was nothing for it but to borrow the

complete copy from the Royal Society and replace the missing plates by a special photographic process which, if its results are not equal to the definition of the originals, are all that is needed for the purposes of work. But if, as Ruskin would say, no man with a heart in his breast can be satisfied with copies, what prospect was there of duplicates becoming available? Many years later a Cambridge bookseller listed an assortment of medical publications bound together, price £1, the contents not being specified. On these occasions I am often tempted to gamble on the chance of some important work being included, and as usual with gambling I generally lose my money. Here, however, was the rare exception, and I was delighted to find among the rest *the third volume of the Acta Medica,* but again my luck was out since one plate was still missing. More years of waiting followed, and then final success. The chapter of Exeter Cathedral decided to dispose of certain old medical books in their library. One of them was volume 3 of the *Acta Medica,* and so my set was at last completed.

The repair of old books is a highly skilled craft in which there are only a very few practitioners of the first order. In early days I had bought for £5 a copy of the first edition of Vesalius (1543). The portrait was missing, and the lower right-hand corner of the engraved title had been torn off. The restorer in this case was a woman who grafted on to the page a piece of paper of the same date, quality, and colour. The missing engraved portion was then filled in line for line by an artist working from a perfect copy. The result, which I usually showed to visitors, was astonishing and almost unbelievable, but if the page were held up to the light the repair union could be recognized. Having sold this copy for £150, I acquired a superb exemplar for £325 (now it would be £750), but in this copy one of the folding plates was in such a deplorable state that I despaired of ever being able to save it. However, the arch-priest of this most subtle craft, Mr. Drescher of Harrow, took it in hand with such outstanding success that I still have something with which to astonish my visitors.

When I inherited from Sir D'Arcy Thompson's library a copy of Rouveyre's edition of Leonardo da Vinci's MS. notebook on the flight of birds, it included a facsimile reproduction in colour of the MS. as it then was. There were 18 leaves completed by Leonardo, but 5 of them had been torn out and lost. All, however, were subsequently recovered and the complete original MS. of 1505 is now in the Royal Library in Turin. This facsimile is the finest example of its kind I have ever seen, and when you hold it in your

hand and note the soiled patches, various discolourations, colour offsets, and damp stains, all meticulously reproduced, it is difficult to believe that you are not handling the original itself. And what makes it still more surprising, the date is 1893. Nevertheless, a glance at the copy with a lens reveals the type of technique employed.

All collectors of the earlier literature of natural history should have acquired some skill in bookbinding and be able to carry out minor repairs themselves. Also folding plates are frequently badly torn, but the separated parts can be skilfully assembled and pasted together by their edges. More rarely plates are missing, and it is hence useful to be familiar with the methods employed by the successful copyist. Most forgeries are easily recognized, and none can defeat expert examination. The nearest approach to success I have seen were some very clever Paris forgeries of Rembrandt etchings. The original plates being etchings made the production of high grade forgeries possible, but they did not survive the application of micro-metric methods of detection.

In 1938 I reprinted two little tracts published by the Amsterdam anatomists in 1667 and 1673 *(Observationes anatomicae selectiores)*. Apart from a copy of the first tract in the public library of Preston, the complete work can be seen in this country only in the British Museum, the Bodleian Library, and the Hunterian Library in Glasgow. I once exhausted all my resources in an attempt to obtain a copy for my own library, which involved writing over two hundred letters and postcards to possible avenues of supply. The result was complete failure. It is a common belief amongst the well-affected that a rare book will mysteriously appear out of the blue if only you wait long enough for it. And since that has happened more than once in my lifetime I am still hopeful that this rarest of books will be duly recorded in my catalogues before our little play is played out.

Ancient Peruvian Medicine

LEON GOLDMAN* AND ALAN R. SAWYER**

THE study of ceramics is one of the highly important phases of the archeology of ancient Peru, providing the archeologist with his most reliable diagnostics with which to pinpoint the chronological position of sites within the more than three thousand years of pre-Columbian Andean history. The evolution of styles, their intermingling and succession, is a subject bewildering to the uninitiated. (The accompanying chart shows, in greatly simplified terms, the main cultural periods and their relative chronology.) It is sufficient to observe here that nowhere in the Americas did the art of the potter achieve and maintain such a high degree of technical and artistic excellence or exhibit such a wide range of subject matter. Of special interest to the physician are many representations of individuals suffering from disease.

The ancient Peruvian cultures gave unequal emphasis to the portrayal of the sufferings of man. By far the most important group of such material comes from the Mochica culture which flourished on the North Coast of Peru about two thousand years ago. Their realistically modelled head and figure vessels quite often display medical aspects with remarkable accuracy. With a modified and less precise realism, the cultures following the Mochica on the North Coast occasionally portray subjects of pathological nature. On Peru's South Coast, the Nazca peoples, contemporaries of the Mochica, rarely produced modelled figures showing definite symptoms. In general, the highly symbolic and abstract style in which their superb polychromed ceramics were fashioned discourages medical prognostication. Similarly, other ancient Peruvian cultures produced relatively few ceramics of medical interest. The total of such material is, however, prodigious.

The coastal area of Peru is an extremely arid desert cut at odd intervals by river valleys to which ancient and modern habitation has been limited. The state of preservation of materials found in

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** The Art Institute of Chicago.

Courtesy of the Art Institute of Chicago

FIG. 1. Two Mochica figure jars representing an achondroplastic dwarf and arm amputations. Nathan Cummings Collection.

Courtesy of the Art Institute of Chicago

FIG. 2. Five Mochica "stirrup spout" vessels. Top row: Man with tattooed face and hare lip—or leishmaniasis (?); skull; two death-head figures in erotic pose. Lower row: Man chewing coca and another suffering from acromegaly. Nathan Cummings Collection.

Courtesy of the Art Institute of Chicago

FIG. 3. Six Mochica ceramics and one Nazca (lower left). Top row: Man chewing coca, right leg amputated; man with rolled mat and bedpan (?) whose nose and ears are partly eaten away and who has deep gouges in his skull—traumatic (?); hunchback; man with nose and feet missing (lepromatous leprosy?), tattooing on face, arms, and legs. Lower row: man with distorted hands and face, chewing coca; portrait of man with nose and lips missing—punitive mutilation, healed uta, or congenital (?) syphilis; Death, mother and child. Nathan Cummings Collection.

Courtesy of the Art Institute of Chicago

FIG. 4. Seven Mochica vessels. Top row: Composite of grimacing face and animal figures—not dipsomania, but diety concept; pile of human corpses and animals and birds; head of round-faced infant, or obesity (?); skeletal figure. Lower row: Senility; man with one eye; dance of skeletal figures—death or emaciation? Art Institute of Chicago Collection (Gafforon).

Courtesy of the Art Institute of Chicago

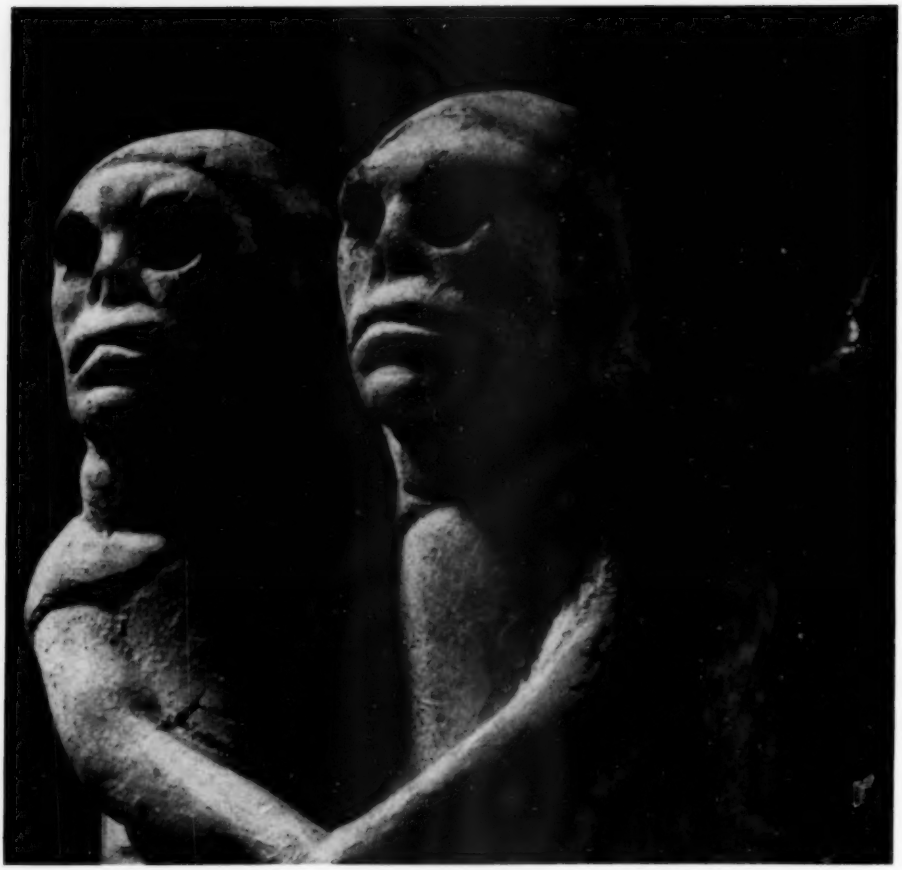
FIG. 5. Seven Mochica ceramics. Top row: Figure with eyes covered holding hearts (?) in right hand and amputated arm in left—priest figure after sacrifice, or first cardiopathologist on record (?); man with puffy face wearing kerchief—not necessarily with toothache or nephrotic syndrome; peripheral facial palsy; man with eyelids, nose and lips missing—disease, mutilation, or death? Lower row: Woman with protruding teeth (an attribute of Mochica diety) and lice on body; a pregnant woman; and an obstetrical (?) examination. Art Institute of Chicago Collection (Gaffron).

Courtesy of the Art Institute of Chicago

FIG. 6. Inca ceramic depicting removal of parasite or thorn from foot. Art Institute of Chicago Collection (Gaffron).

Courtesy of the Art Institute of Chicago

FIG. 7. Mochica blackware vessel depicting mother and child—mutilation, death, or third generation syphilis? Nathan Cummings Collection.



Courtesy of the Art Institute of Chicago

FIG. 8. Detail of ceramic in upper right of Figure 2, showing powerful realism of the two death heads. Nathan Cummings Collection.

the ancient cemeteries on the desert fringes is remarkable (even vegetable foods and body lice are found). Therefore, added to the impressive body of ceramic material, the physician may examine with interest human remains in which evidence of tattooing and skin eruptions occasionally survive. Skulls sometimes display skillfully executed trepanation[11,12,13] (especially in the early Paracas cultures of the South Coast), and occasionally evidence of bone

CHART

(After Sawyer's Handbook on the Nathan Cummings Collection)[4]

	North		*South*	
Dates	*Highland*	*Coastal*	*Highland*	*Coastal*
1532-1821		SPANISH COLONIAL		
1470-1532		INCA EMPIRE		
1000-1470	Hamachuco	Chimu	Inca	Ica
700-1000		TIAHUANACO EMPIRE		
400-700	Recuay B	Late Middle Mochica	Huari	Nazca
AD-400	Recuay A Negative Style	Early Mochica	Classic Tiahuanaco	Paracas Necropolis
BC 400-1000	?	Salinar	Pucara	Paracas Cavernas
400-	Highland Chavin	Cupisnique Chavin Horizon	?	?

disease appears to be present. Among the many beautifully wrought objects of metal, stone, bone, and other materials are found instruments which may have been designed for surgical use, and the textiles which have amazed the world by their beautiful designs and varied and intricate techniques, include pressure-type bandages. A "ward round" among ancient Peruvian materials can, in fact, be just as fruitful and much more fascinating than wandering through a gallery of medical moulage exhibits of years ago, or viewing colorful panels of kodachrome or stereo pictures in medical meetings of today. Pathological materials of ancient Peru offer us a textbook of ancient American medicine which is unparalleled among the prehistoric peoples of the world.

The Art Institute of Chicago has probably the most distinctive collection of ancient Peruvian art objects outside of the National Museum and most extensive private collections in Peru itself. This wealth of material is comprised mainly of the famous Dr. Eduard Gaffron Collection, purchased by the Institute, and the equally renowned Nathan Cummings Collection (formerly the B. J. Wassermann Collection) which is on indefinite loan. The aesthetic importance of these two great collections has been described before.[1-4] This report is concerned with the pathological aspects of the ceramic materials.

One must be aware of certain distinct limitations before attempting diagnosis by the hazardous, though intriguing, technique of inspection.[5-10] Differential diagnosis must take into account our incomplete knowledge of ancient Peruvian habits of dress and stylistic conventions, for what we are tempted to interpret as a turban tumor or metastatic bone lesion may have been intended as a distinctive hair style. We must keep in mind that the ancient craftsmen expressed what is to us a bewildering complexity of mythological and religious concepts. What they intended to represent as a demon may look to us like dipsomania. Next, there is evidence to indicate that punitive mutilations suffered by these people and other traumatic lesions of the face may be confused with leishmaniasis, for example. Finally, unless an object is obviously a study in disease, such as vessels showing purulent conjunctivitis with pus dripping down the cheek, one must consider the interests of the observer. This is true especially in the field of the supposed granulomata where an observer may be anxious to prove its existence in one of the periods of pre-Columbian medicine. For this reason, our clinical classifications listed below incorporate some of the other possible interpretations suggested by archeologists which may help calm the excited ceramic-slanted clinician. This is not to imply that archeologists in anyway fail to appreciate the medical aspects of these materials. With their broader view, they are, however, less inclined to make final judgments when several possibilities exist.

Though our classifications are based on categories present in the collections of The Art Institute of Chicago, they should prove adequate to contain most of the "medical pieces" in other museums.

Classification of Pathological Aspects in Ancient Peruvian Ceramics

1. *Death.* Skeleton-like figures may at times represent emaciation. The practice of punitive mutilation (including flaying of the face) may lead us to interpret some skull-like representations of surviving victims as death-heads.

2. *Diseases of the eyes.* Blindness in one or both eyes. The loss may have been suffered through battle injury or punishment rather than disease. Closed eyes may merely represent sleep or trance. Tumors about the eyes are sometimes represented (not to be confused with early stylization which resembles swollen eyelids).

3. *Skin lesions,* with papules, nodules, ulcerations. These are hard to diagnose with certainty. They may be leishmaniasis, leprosy, or primary syphilis (though no positive evidence has been found of the latter; bone pathology is sometimes convincing for syphilitic osteo-periostitis). Here, punitive mutilations may be misinterpreted and coca chewing, tattooing, and face or body painting may be taken for skin eruptions.

4. *Nerve injury,* including peripheral facial palsy. Head injuries were common among warriors whose main weapons were clubs and maces. This fact undoubtedly accounts to a considerable extent for the necessity of developing the science of cranial trepanation.

5. *Congenital abnormality*—hare lip, achondroplastic dwarf, hunchback, etc.

6. *Obesity and emaciation.* In their realistic ceramics, the Mochica in particular represented all physical types.

7. *Character type, mood, senility, and psychosis.* In this category, the present-day diagnostician tends to superimpose his own cultural interpretations. He is also prone to classify the ancients' representations of demon and diety in modern psychological terms.

8. *Eroticism*—a category which has given Dr. Kinsey and his Institute for Sex Research a unique opportunity to study the sexual behavior of pre-Columbian peoples.

9. *Pregnancy and childbirth* in all its stages.

10. *Medical examination and treatment*—shaky ground for interpretation. What may appear to be bedside manner may have been intended as the preparation of a victim for sacrifice.

With due recognition, then, of the limitations of diagnosis through inspection the student of medical history will find much of interest in the portrayal of pathology in ancient Peruvian ceramics. Pottery of this type forms a relatively small part of the extensive collections at The Art Institute of Chicago. It is usually not well represented among the pieces on display as works of art; however, interested professional groups or individuals may arrange to see them in the museum's storerooms.

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The Genesis and Impact of the Medical Lobby: 1898-1906

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SINCE the conclusion of World War II, few lobbies in Washington have been the subject of more controversy than that of the American Medical Association. Decrying "socialized medicine," the A.M.A. has opposed the further entrance of the Federal government into the field of personal medical care. Its determination and political skill have won the respect of both its friends and foes. Some of its critics have charged that the success of the A.M.A. in politics is due to sheer economic advantage.[1] But the power of the medical lobby is the result of more than fifty years' effort to develop a militant political organization capable of securing public health legislation. The medical lobby since 1901 has followed a program dedicated to two goals: the improvement of the public health and the furtherance of the interests of the medical profession.

This paper traces the factors leading to the organization of the A.M.A. Committee on National Legislation through its first half-decade of activity. The Committee was established not only as a negative force to oppose antivivisection bills and higher postal rates for medical journals, but to propose national sanitary laws, better medical care for the armed services, and higher standards of medical education. By 1906 the A.M.A. had successfully battled with President Theodore Roosevelt and his administration to provide better medical care to workers constructing the Panama Canal; it had triumphed over the powerful distillers' trust and the right wing of the Republican Party in order to pass the Pure Food and Drug Act of 1906. In a seven-year period a politically powerless profession was organized into one of the most politically potent grass-roots lobbies in the United States.

The definition of a lobbyist may vary widely; but certainly there have been lobbyists almost as long as there have been legislators. However, it would be well to define a lobbyist as an organization or individual functioning primarily to influence legislative

* Portland, Oregon.

[1] *The decline of the medical profession in public esteem.* Milwaukee, Milwaukee Labor Press, 1955, p. 13.

or administrative activities. The coming of the twentieth century marked the era of American industrialization. Small organizations were becoming big; the trusts and the labor unions were growing. The stigma which has been attached to the word *lobbyist* stems from these years when the Congress and the state legislatures were held in control by the financial gifts offered by railroads, insurance companies, oil producers, public utility companies, and even school book manufacturers. At the other extreme, there were organizations of large memberships including the Anti-saloon League, the Farm Bureau Federation, and the American Federation of Labor which had been actively lobbying since 1881.[2] The embryo medical lobby was a unique innovation. It was neither an economic giant with ample funds to buy influence nor a numerical giant with enough votes to command attention.

The medical lobby grew in reaction to the conditions of the nineteenth century when American medicine was ridden by many different "sects," each with its own theory of the nature and the treatment of disease; few physicians were then interested in studying disease processes as a logical basis for treatment. In addition, large numbers of quacks roamed the nation practising their deceptive arts. There were few state laws regulating medical practice, while hospitals and clinics operated with only their directors' wisdom and conscience dictating standards of professional care. Since medical schools were not accredited by any competent body, the quality of undergraduate medical education varied widely. In addition, there were few journals to communicate the results from laboratory and clinical research which were to form the basis of a more scientific practice of medicine during the twentieth century. In an attempt to improve upon these conditions physicians throughout the nation banded together in 1847 to found the A.M.A. as an independent scientific and educational organization. Its founder, Dr. Nathan Smith Davis, decried not only the poor education then offered by medical schools but the inadequate preparatory education of their entering students.

By the turn of the century, the A.M.A. had begun to influence medical practice in America. Standards of ethical behavior had been adopted and were being enforced among its own membership. Some medical schools, notably Northwestern, Harvard, Yale, University of Michigan, and especially the Johns Hopkins, had responded to the demand for a more thorough education. State

[2] Logan, E. B. Lobbying. *Ann. Amer. Acad. pol. soc. Sci.*, 1929, *supplement to vol. 114*, 3-45.

licensing boards were establishing minimum standards for physicians.[3] The Carnegie and Rockefeller Foundations had not yet been founded; the *Flexner Report* had not yet been written. In 1900 *The Journal of the A.M.A.* had twice as many subscribers desiring education as the Association had members desiring organization. Politically as well as numerically the A.M.A. was still weak. Although local or state medical societies would lobby for needed legislation or the officers of the A.M.A. would attempt to influence Congress on particular bills, there was no organized national medical lobby, for in fact the A.M.A. could claim to represent only nine per cent of the medical profession.[4]

In 1898 two issues arose to strengthen interest in improving the Association's influence with the legislators. The A.M.A. convention of that year determined that the need for uniform sanitary laws required the establishment of a National Department of Health and proposed that the agency be headed by a physician rather than a politician. Secondly, Dr. William H. Welch and others warned the convention of an antivivisection bill introduced by U.S. Senator Galinger. Although the bill proposed to regulate vivisection only in the District of Columbia, Welch feared that its passage would encourage similar restrictive laws in the States as well as prevent medical research and education in the nation's capital. The A.M.A. passed resolutions stating its position on each of these debates. Still the Editor of *The Journal of the A.M.A.* was sceptical:

> Resolutions are all very well in their way, but it is questionable if they will have as telling an effect on a vote of the United States Senate, as an argument by individual members of the Association to individual members of the Senate.[5]
>
> If it is understood that the *whole* profession really take an interest in the matter the local lobbyists at Washington will be powerless to stop the reform.[6]

He saw that the medical profession must go to battle as a national grass-roots movement if it was to defeat its political foes.

But even grass-roots movements need representation in Washington, so Welch went before the Senate Committee on the District of Columbia to oppose the antivivisection bill. There he was put in the uncomfortable position of having to oppose another

[3] Welch, W. H., *Papers and addresses by William Henry Welch*. Baltimore, Johns Hopkins Press, 1920, vol. III, pp. 120-121.
[4] Keen, W. W. Presidential address. *J. Amer. med. Ass.*, 1900, *34*, 1445.
[5] Editorial. *J. Amer. med. Ass.*, 1898, *30*, 153.
[6] *Ibid.*, p. 222.

physician, Senator Galinger, on a matter of scientific judgment. The Committee recommended passage of the bill on the basis of its inadequate technical knowledge, ignoring the protests of more than one hundred scientific societies including, in addition to the A.M.A., the National Academy of Sciences, the American Association for the Advancement of Science, and the American Public Health Association.[7] Fortunately, the bill did not become law as it was later forgotten by another Committee.

Yet this aborted legislation had an important result in that the Ohio State Medical Society saw the danger of the antivivisection bill. Dr. L. B. Tuckerman of that group suggested that the A.M.A. appoint a National Committee on Medical Legislation. Because of the time required to travel at the turn of the century, he suggested that the Committee have three members, one living in Washington, one in Baltimore, and one in Philadelphia in order that they be able to attend the Congress on short notice. He also proposed that each of the state medical societies appoint a Committeeman on National Legislation whose duties would include making contact with the Congressmen of his own state besides advising the three-membered Committee. Tuckerman's suggestions were adopted by the Ohio State Medical Society on 5 May 1898.[8]

In June 1898 Dr. William H. Humiston took Tuckerman's proposal to the floor of the A.M.A. annual meeting. There, the plan to create a Committee on National Legislation was referred to the Business Committee. However, the Business Committee could not "see its way clear to the efficient operation of such a Committee without a considerable expenditure. . . ."[9]

In the meantime interest in politics was rising among the officers of the A.M.A. When the Association met the following year, its President, Dr. Joseph B. Mathews, discussed the advantages of making Washington the headquarters of the A.M.A.: ". . . it would be in touch with the legislative department of the Government, and it can readily be seen of what value this would be in securing much-needed reforms in a medical way."[10] The A.M.A. was thus moving slowly toward an interest in legislative affairs.

But Tuckerman was not content to wait for the Association to move its headquarters to Washington. In fact, he was not content

[7] Welch, W. H. Objections to the antivivisection bill now before the senate of the United States. *J. Amer. med. Ass.*, 1898, *30*, 287.

[8] Official minutes of the general sessions. *J. Amer. med. Ass.*, 1898, *31*, 84.

[9] *Ibid.*, p. 95.

[10] Mathews, J. B. Presidential address. *J. Amer. med. Ass.*, 1899, *32*, 274.

to wait one year for the regular procedure of a Constitutional amendment to establish the Committee on National Legislation as one of the A.M.A.'s standing committees. Although he introduced a Constitutional amendment, he also introduced a resolution calling for an interim Special Committee on National Legislation since special committees could be appointed without a one-year waiting period.[11] This time Tuckerman's ideas were accepted: the Board of Trustees recommended that the plan be passed at the convention of 1899. After the resolution was approved by the membership, the following eminent physicians were appointed to the Committee: Dr. H. L. Johnson, Dr. W. H. Welch, and Dr. W. L. Rodman. Two hundred and fifty dollars were appropriated to launch the Committee.[12]

On 10 November 1899 these three held a preliminary meeting in Washington to map out the policy of the Committee for the coming year. Two months later, Dr. Johnson, their Chairman, requested the state medical societies, the Army, the Navy, and the Marine Hospital Service to appoint representatives to a Conference on National Legislation. The date of the Conference was not announced, because the Committee wished to allow the state medical societies time enough to consider these issues:

1. The proposal put forward by the A.M.A. for the creation of a National Department of Public Health.
2. The request that Congress pay for the publication of the *Index Medicus*.
3. The means to defeat the antivivisection bill.
4. The proposal to make uniform the various state medical practice acts.[13]

This consultation with local and armed services representatives by the Committee on National Legislation was well calculated to enlarge its political influence. Tuckerman demonstrated how such state delegates could add to the strength of the Committee on National Legislation. He suggested that the meetings of the Conference be held in the afternoons so that the delegates could spend their mornings informing their respective Senators and Representatives of their attendance at the A.M.A. Conference:

> That Committee would then represent in the minds of the Congressmen, not merely the American Medical Association, but the organized medical profession of the United States. It could see that the proper bills were

[11] Official minutes of the general sessions. *J. Amer. med. Ass.*, 1899, *32*, 1337.
[12] *Ibid.*, p. 1371.
[13] Report of committee on national legislation. *J. Amer. med. Ass.*, 1900, *34*, 1547.

introduced, that they were pushed before the proper committees. . . . This move of the American Medical Association makes it possible for the opinions of the medical profession to exercise a steady pressure on Congress for the public good. It furnishes a means by which the consensus of the mature judgement of the medical profession of the United States can find authoritative expression, and puts it out of the power of irresponsible physicians, with time on their hands and "money to burn" to air their fads before Congressional committees under the guise of medical opinions.[14]

Because the Conference was called for 1 and 2 May 1900, the vivisection bill had to be protested earlier in the legislative year. A.M.A. President William W. Keen bypassed his Committee on National Legislation and personally organized the opponents of the bill for a hearing before the Senate Committee on the District of Columbia on 21 February 1900. After addressing the Senators in his rôle of spokesman for the A.M.A., Keen introduced an array of famous physicians including Drs. Osler, Welch, Bowditch, Jacobi, and Howard Kelly, all of whom spoke in opposition to the bill.

At the hearings it became apparent that the bill was not strictly an antivivisection measure but an over-regulate-almost-to-the-point-of-prohibit vivisection proposal. Entitled, "A Bill for the Further Prevention of Cruelty to Animals in the District of Columbia" (S. 34, 56th Congress, 1st Session), it would require that experiments which gave pain to a vertebrate animal could not be performed without a license from the District Commissioners. Every application for such a license would require the signature of three licensed physicians and one medical school professor. Illustrative of the restrictions imposed by the bill, chloroform and ether were the only anesthetics recognized, the use of opiates was prohibited, and inspectors were to make unannounced raids on the medical schools. The bill contained the manifest danger of prohibiting specific forms of medical and surgical research. The interference and nuisance of its enforcement probably would have closed the District's medical schools. Moreover, the passage of any vivisection measure by the Congress might have greatly influenced the state legislatures to pass even more restrictive legislation.

Senator Galinger appeared in three rôles: author of the bill, chairman of the committee hearing the bill, and a retired physician. From this vantage ground he could take a very friendly tone while still advancing his side of the argument. When Keen spoke of the rôle of dog surgery in neurophysiology and the development of

[14] Tuckerman, L. B. Correspondence. *J. Amer. med. Ass.*, 1899, *33*, 1238.

surgical procedures, Galinger replied, "We recognize you, Dr. Keen (I certainly do) as a man of profound learning in the medical profession and as speaking, in a sense, by authority."[15] Galinger's main efforts were directed toward dividing this "authority." He questioned Dr. Henry P. Bowditch of the Harvard Medical School about the dissenting opinion held by Dr. Henry J. Bigelow also of that institution. Bowditch retorted: "He did object to vivisection, but, Mr. Chairman, he knew nothing about modern physiological research. He had derived his impressions from what he had seen in the French veterinary school at Alford forty years before."[16]

The Senator was not the only medical witness against vivisection. The appearance of several physicians favoring the regulatory bill confirmed the need for the A.M.A. to present an authoritative consensus of expert medical opinion. One retired physician, a Dr. Albert Leffingwell of Aurora, New York, attacked Keen for an editorial in *The Journal of the A.M.A.* and challenged the entire principle of A.M.A. political activity.[17]

> It seems to me, Mr. Chairman, that the author of that editorial took upon himself a very grave responsibility. He was addressing the whole medical profession in this country, a body of over a hundred thousand men; and he was inflaming their minds against this bill—one which many of them would never have occasion to read—as a measure, the object of which was the "prohibition of vivisection." He appeals to them to write letters to Senators, to hold meetings of medical societies to pass resolutions of protest against this measure. I believe one speaker [Dr. Hare] has referred to these letters and these resolutions, and that he put the question to the Committee whether "you have not received protests from all parts of the country against this bill?" Why of course you have received protests. This editorial explains why.[18]

In spite of the opposition, Dr. Keen's protest was successful. The bill was never reported out by the Senate Committee despite the fact that its Chairman was the author of the bill. Perhaps the Committee was also influenced by a report submitted to the Commissioners of the District of Columbia by Dr. William C. Woodward. Woodward, as Health Officer for the District, had been asked by the Commissioners for his expert opinion on the Vivisection Bill. On 26 January 1900 he declared the bill vague, un-

[15] Hearings before the United States Senate Committee on the District of Columbia on Senate Bill 34, 56th Congress, 2d Session. *Senate documents*, 1901, vol. XXVI, no. 337.

[16] *Ibid.*, p. 57.

[17] Editorial. *J. Amer. med. Ass.*, 1899, *33*, 1618.

[18] *Op. cit.* (15), p. 112.

necessary, and interfering.[19] It is interesting to note that eleven years later Woodward became the first head of the A.M.A. Bureau of Legislation. Nevertheless, it is President Keen who deserves most of the credit for the defeat of Senator Galinger's bill.[20]

Thus, when the Conference on National Legislation met in May 1900, the fifteen delegates who appeared were told by Surgeon-General Sternberg that the antivivisection measure was "practically dead." Still, he thought the issue would be raised again and that the A.M.A. should be ready to act. Then the Surgeon-General proceeded to do a little "killing" himself, by asserting that there was little need for the *Index Medicus* because many medical journals had begun to publish more adequate indices.[21] Acting on his advice, the Conference crossed the *Index Medicus* off its agenda; it remained unpublished until 1903.[22]

As an influential proponent of medical progress within the government, the Surgeon-General went on to speak in favor of three bills affecting the Army Medical Service. These called for an increase in the number of physicians in the regular Army (H. 4483, 56th Congress, 1st Session), the creation of a medical reserve program (S. 4274), and a bill to equalize the leave-of-absence and sick-leave benefits of assistant surgeons with those of other commissioned officers (S. 4200).[23] The A.M.A. wanted to support all of these bills in order to raise the status and privileges of military physicians. At that time Army doctors suffered under a pay and promotion system dating back to the war between the states. At the turn of the century Army doctors still lagged behind line officers in rank and pay. In the eyes of the A.M.A. their status did not reflect the gains in scientific medicine made during the preceding quarter of a century and reflected badly on the status of civilian physicians as well.

The Conference sent separate letters to the Chairman of the House Committee on Military Affairs in support of each of the military bills. The preamble of those letters follows:

"We are directed by the Committee on National Legislation which represents the American Medical Association and each state and territory medical society of the United States, to inform you that. . . ."[24]

19 Woodward, W. C. Antivivisection: a review of the proposed law. *J. Amer. med. Ass.*, 1900, *34*, 381-383.
20 Editorial, *J. Amer. med. Ass.*, 1900, *34*, 1492.
21 *Op. cit.* (11), p. 1548.
22 *Ibid.*, p. 1550.
23 *Ibid.*, p. 1548.
24 *Ibid.*, p. 1552.

Of the three, only the leave benefit bill was finally to become law. This astute pointing-up of its grass-roots character was to become a hallmark of A.M.A. lobbying activity.

The Conference then turned to the issue of a National Department of Health. The pending Bill (S. 3433) had been dubbed "Spooner" for the Senator who had introduced it; actually it had been written by Dr. U. O. B. Wingate, the Wisconsin delegate to the National Conference. Many of the delegates thought the Spooner Bill defective because the nation's chief Health Officer, as a member of the President's Cabinet, would be a political appointee. Tuckerman commented that "when an officer is chosen, not for his ability but for the support he would bring to his party, his efficiency as a health officer would be absolutely *nil.*"[25] Welch agreed with Tuckerman and suggested that it would be better to experiment with a Bureau instead of a Cabinet-rank Department. When Surgeon-General Wyman of the Marine-Hospital Service chimed in that the Spooner Bill would disrupt his own efficient organization, all support for the bill vanished. Delegate Wingate was forced to return home with no pledge of A.M.A. support. Subsequently, the bill was never reported out by the Senate Committee hearing it.

Surgeon-General Wyman then asked the support of the Conference for a pair of identical bills to increase the powers of the Marine-Hospital Service (H. 11139 and S. 4171). These bills gave quarantine officers the power to make arrests and to administer oaths. The Conference ordered a subcommittee of five to write and visit the Senate Committee on Public Health and National Quarantine.[26] The bill passed at the following session of the Congress.[27]

At the next annual meeting of the A.M.A. in June 1900, Johnson, Welch, and Rodman reported on the activities of their committee. They suggested that the A.M.A. Constitution be amended to make the Committee on National Legislation a standing committee. They added that an annual conference of delegates from the state medical societies would "awaken interest in National medical affairs and will give the legislators a medium for better understanding the wishes of the country at large. . . ."[28] Then the original Tuckerman plan, with very slight modifications, was

25 *Ibid.*, p. 1549.
26 *Ibid.*, pp. 1551-2.
27 *Congressional record*, 56th Congress, 2d Session, 1901, p. 3523.
28 Official minutes of the general sessions. *J. Amer. med. Ass.*, 1900, *34*, 1552.

adopted by the Association: the Committee on National Legislation had become a permanent part of the A.M.A.[29]

But even with its new status as a standing committee, the Committee on National Legislation was not assured of success. When it called for the Second Annual Conference on National Legislation, to be held in Washington on 20 and 21 February 1901, attendance was disappointingly low. Fourteen states and the District of Columbia were represented. Eight states had each nominated a delegate and an alternate, none of whom actually attended the Conference, while twenty-six state medical societies did not consider sending a delegate at all. The Councillors of the Massachusetts Medical Society voted to "lay on the table" the question of sending a delegate, but never brought the issue up again.[30] The reasons for the lack of attendance were pointed out by Dr. D. S. Reynolds of Kentucky.

> I had not the privilege of being with you last year. I have learned a great deal since I came here, and I see now more fully the value of this committee and when I return home I will endeavor to present a synopsis of the work to our state society. . . . A great many state societies have no knowledge of this committee and its possibilities. . . .[31]

To combat this lack of awareness the Conference report urged the need of organization within the respective state societies "to bring the pressure of the entire medical profession"to bear on the Congress.

> The other day when a question came up in Congress about a particular clause in a certain bill, it was stated that Workingman's Union No. so-and-so, had drawn the bill, through their attorneys, and that it would undoubtedly pass. It seems to your committee that our profession should be given the same recognition.[32]

It is to be noted that this statement was made in 1901, a time when the growth of the American trade union movement had hardly begun.

The Second Annual Conference reviewed the progress of the various military and Marine-Hospital Service Bills. There was also some discussion concerning the possibilities of making the state medical practice laws more uniform and of increasing licensing reciprocity between the states. However, the most important outcome of the Second Annual Conference was its successful effort to

29 *Ibid.*, p. 1559.
30 Report of committee on national legislation. *J. Amer. med. Ass.*, 1901, *36*, 1636.
31 *Ibid.*, p. 1638.
32 *Ibid.*, p. 1637.

influence second-class postage rates. This move marked the first excursion by the A.M.A. into the field of economic, rather than primarily medical, lobbying activities.

The issue arose because a Commission appointed by the Congress to revise the postal laws had deleted two words in the revision which would have increased the cost of distributing *The Journal of the A.M.A.* by $30,000 a year.[33] The Postal Commission claimed that they had made "no radical changes" and had merely attempted to "harmonize various provisions" of the existing law which allowed weekly magazines to be mailed at the bulk rate of one cent per pound, while newspapers and monthly periodicals paid one or two cents per copy.[34] In dropping the words "excepting weeklies" from the section (152) specifying mail rates on a per-copy basis, the Commission concluded: "There seems to be no reason why weekly newspapers should be given privileges not granted to other publications."[35]

When the exemption of weeklies from the per-copy rates was debated on the floor of the House, Chairman Loud of the House Post Office Committee defended the Postal Commission's deletion by charging: "It has been intimated that some weekly newspaper . . . succeeded in getting the provision injected into an appropriation bill during the closing hours of a short session."[36] The House upheld Loud with the result that the weeklies were threatened with higher postal rates when H.R. 13423 was passed and sent along to the Senate late in January 1901.

Against this background Tuckerman brought the postal issue before the Conference on 20 February. "There was a Commission appointed to codify the postal laws and some things were added. . . . The daily papers have had it in for the weekly papers for years," he argued, pointing out the increased postage that *The Journal* would have to pay.[37] Dr. Reynolds of Kentucky rose to say that he was forced to return home that afternoon, but that he would write a letter of protest to Senator Lindsay of his home state. Rather than have the postman deliver it, Tuckerman chose to take the letter to Senator Lindsay in person. Here Tuckerman takes up the story and draws his own conclusions:

[33] *Ibid.*, p. 1639.

[34] Report of the Commission to Revise the Postal Laws. *House Reports*, 56th Congress, 2d Session, 1901, no. 2272, p. 2.

[35] *Ibid.*, p. 10.

[36] *Congressional record*, 56th Congress, 2d Session, 1901, p. 1151.

[37] *Op. cit.* (30), p. 1639.

> After adjournment yesterday I went to the Senate, with a letter to Senator Lindsay of Kentucky, from Dr. Reynolds. I pointed out to him the clause which related to medical journals, and other periodicals. He took it to Senator Chandler, who said the bill would not go through with that proviso —we can be positively assured that it will not go through that way. Incidentally, I want to emphasize the remarks of General Sternberg. The only way to secure legislation is for each one of us to see our Senator and Representative personally. We thought we were beaten in Ohio. The osteopaths had secured, by treating legislators' wives, the support of one of the members of the State Senate, and it looked as though he had a majority of the votes, and could bring it up at any time. We called up his family physician, who took the first train for his home. They had lunch together during which they had a heart-to-heart talk. He said to the legislator, "Thomas, you want to go to Congress? If you bring up this bill there are eighteen votes against you." Thomas dropped the bill. The purpose of this organization is to get a firm grip on every Congressman's appendix. I think if we carry it to the states and organize on the New York plan, in five years we can bring from any district enough pressure to bear on each Congressman.[38]

Subsequently, the postal revision bill was snagged in Committee on this and similar issues and was never reported to the Senate as a whole.

To sum up the activity of the Standing Committee on National Legislation during the first year of its existence, it had successfully used its influence in connection with the following bills: the equalization of Army physicians leave allowances; extending the quarantine powers of the Marine-Hospital Service; and the defeating of an increase in postal rates for weekly magazines. However, the Committee was unsuccessful in persuading the Congress to raise the pay and status of military physicians.[39]

In June 1901 the annual meeting of the A.M.A. ratified a new constitution which was to transform the A.M.A. into the central organizing force of American medicine. The general plan was borrowed from the British Medical Association: membership in the national organization was to be made dependent on concurrent membership in county and state medical societies. The county medical societies were to remain entirely democratic organizations with every member physician having the right to vote. The county would then elect a representative delegate body to the state organization. At the national level, representation was to become the responsibility of delegate bodies whose members were to be elected by the state medical societies. The A.M.A. House of Delegates was thus to become the primary medical power in America. Much of this power was to be used wisely to improve standards of

38 *Ibid.*, p. 1640.
39 *Ibid.*, p. 1635.

medical education, further research, and promote needed medical legislation.[40]

The new Constitution reaffirmed the status of a National Committee on Medical Legislation as a standing committee. It also defined the two purposes of the medical lobbyist as the "promotion and preservation of the public health" and "the material or moral welfare of the medical profession."[41] Thus the A.M.A. served notice that both public and private motives should mark the legislative activities of professional groups in a democratic society. The President of the A.M.A., Dr. Charles Reed, noting that votes were the language a Congressman understood best, asserted that the organization should put itself in a position to influence the largest possible number of votes. He added, "Every physician, therefore, should, in a perfectly respectable sense become an active, working politician."[42]

In his presidential address to the Association, Reed devoted most of his time to discussing the political weaknesses of the medical profession. He affirmed Tuckerman's judgment concerning the highly individual nature of past proposals for medical legislation.

> I say the causes of our failure are properly the subjects for careful consideration. I have examined the records of the Association from the date of its organization, and have been profoundly impressed with the fact that memorials, resolutions, or even more definite propositions addressed to the Congress have, for the most part, represented the views or rather the impressions, of the individual members proposing them. They have generally been presented in the general meeting, and have been endorsed without the deliberation essential for wise action; but a deliberation which is simply impossible in the limited time available in our general meetings.[43]

He went on to express the hope that with a stronger organization which was to be the product of the new Constitution and with the efforts of the Legislation Committee, the A.M.A. could look forward to a more authoritative voice than it had exercised in the past.

Reed spoke about the various types of legislation which were sorely needed: a national bureau of health, greater sanitary measures along the nation's waterways, studies of medicinal flora with governmental funds, a pure food law, and a change in the humiliating status of military physicians. On this last point he offers a bit of sociological analysis.

40 Fishbein, Morris. *A history of the American medical association: 1847 to 1947.* Philadelphia, W. B. Saunders Co., 1947, pp. 202-15.

41 Constitution. *J. Amer. med. Ass.*, 1901, *36*, 1646.

42 Reed, C. A. L. Presidental address. *J. Amer. med. Ass.*, 1901, *36*, 1602.

43 *Ibid.*, p. 1602.

When the status of any number of physicians in their representative relationship to society is lowered, the status of the medical profession in general is menaced in corresponding degree. When the Congress, by the enactment of a law, degrades, relatively, the status of an important body of medical men, engaged in the public service, it strikes at the status of every physician in the country. It becomes, therefore, the duty of every member of the medical profession, jealous of his rights, his prerogatives, and the fair name he may leave to his children, to resent as personal between himself and every member of the Congress who voted for this law, the action which cast a stigma upon our profession.[44]

The activities surrounding ratification of the new Constitution and the tone of the presidential address made the atmosphere of the A.M.A. Convention of 1901 more political than medical. Minnesota's Governor Van Sant, arriving on the second day of the meeting to offer a belated official welcome, sensed the political air. "I do not know," the Governor said, "whether it is a good wish to make to a body of doctors or not, but it is a good thing for a lot of politicians. That wish is this: I don't know what you want, but I hope you will all get it."[45]

While the membership of the A.M.A. was being organized along the lines of its new Constitution and the general strength of the organization was being built up, the Committee on Medical Legislation entered into a two-year period of dormancy. There is no indication as to why the Third Annual Conference on National Legislation held only a one-day meeting as late in the legislative year as 10 April 1902. At any event the Conference did not accomplish much. None of the bills which it recommended was finally passed, although all of them were military in character. Two of them (S. 4112 and H.R. 8194, 57th Congress, 1st Session) provided for an increase in the number of physicians in the Navy and the equalization of Navy pay for medical officers, chaplains, and line officers with Army pay for the equivalent grades.[46]

It proved unfortunate that the Conference centered most of its activity on supporting a bill (S. 5213 and H. 13725) to raise the rank of Surgeon-General Sternberg from Brigadier General to Major General in order that he might retire with higher pay and prestige.[47] Sternberg had contributed much to the progress of the Army Medical Department, and the promotion and the pension

[44] *Ibid.*, p. 1601.

[45] Van Sant, S. R. Official minutes of the general sessions. *J. Amer. med. Ass.*, 1901, *36*, 1643.

[46] Special report of the committee on national legislation. *J. Amer. med. Ass.*, 1902, *38*, 1173.

[47] Conference of the committee on national legislation. *J. Amer. med. Ass.*, 1902, *38*, 1024-25.

might well have been approved by the Congress had not two other issues interfered. The first was a growing dislike in the House of Representatives for the process of promoting officers immediately prior to their retirement for the purpose of raising their pensions; the second was a certain amount of opposition to voting for a bill sponsored by the A.M.A. and the state medical societies which would have benefited Sternberg, a former President of the A.M.A.

The Senate version of the Sternberg bill passed within two months of its introduction. When the House Committee on Military Affairs reported the bill out favorably and the time for a floor vote had come, a debate was precipitated. Opposition to the promotion-before-retirement system became especially intense when some Congressmen began to resent the rôle of the medical lobby in the entire affair. A Congressman from Georgia remarked:

> The medical fraternity of my state, in convention assembled, has recommended its passage. So have similar conventions in several other states of the South.[48]

Another Congressman answered:

> My friend from Georgia stated the fact absolutely when he said he was going to vote for this bill, not because this man was going to promote the service of the United States, not because he is entitled to $2,000 a year more for the balance of his life, but because the State Medical Board of Georgia has asked that he be promoted. . . . If our constituents send us here to have State societies direct us how to cast our votes we had better go home and let them send men here who can do their thinking for themselves.[49]

When the question was voted, General Sternberg was denied his promotion. More important, the Committee on Medical Legislation had suffered a public defeat as well.

The only positive accomplishment of the Third Annual Conference was some discussion looking toward the establishment of a National Board of Medical Examiners. The plan was to follow from the efforts of the A.M.A. to establish national standards for medical education.[50] W. L. Rodman who proposed the voluntary National Examining Board described a written and practical examination which would be at least equal to those given by the State Boards. However, he did not envision that specialists or practitioners would be given the same type of comprehensive examination required by the State Examining Boards, because such examinations were weighted in the favor of recent graduates.

48 *Congressional record*, 57th Congress, 1st Session, 1902, p. 6195.
49 *Ibid.*, p. 6196.
50 *Op. cit.* (47), p. 1023.

It is both unnecessary and unfair to examine men of experience and merit upon such branches as chemistry, physiology, histology, etc. Such a board could have discretionary power and give the kind of examination necessary to ascertain if the applicant is competent to practice general medicine and surgery, or even a specialty, if one is recognized as a specialist. Is it fair to expect an ophthalmologist to submit to an examination in obstetrics?[51]

The membership of the proposed National Examining Board was to be nominated by the A.M.A., the American Congress of Physicians and Surgeons, the National Confederation of State Medical Examining and Licensing Boards, and the three Surgeons-General. Thus the over-all plan is seen as a combination of what today is constituted by the National Board of Medical Examiners and the several specialty boards. However, the A.M.A. House of Delegates declined to consider the proposal after receiving a statement by the National Confederation of Medical Examining and Licensing Boards insisting that the national board would have no legal status and thus serve no useful purpose.[52]

There is no mention in *The Journal of the A.M.A.* of an annual conference being held in 1903. But the Committee on Medical Legislation did meet early in the year to protest another anti-vivisection bill.[53] That bill was never given much consideration by the Congress and so the matter was dropped. In the meantime, the lines were forming for the first battle between the A.M.A. and the Executive Branch of the Government; this was an effort to support Colonel William C. Gorgas in his fight against malaria and yellow fever at the site of the Panama Canal.

"An Act to provide for the construction of a canal connecting the waters of the Atlantic and Pacific Oceans" (The Spooner Act) was signed by President Theodore Roosevelt on 28 June 1902.[54] The act authorized the President to appoint an Isthmian Canal Commission to construct a canal either through Colombia or Nicaragua. Almost two years elapsed before the Commission was appointed because of the political and economic complexities involved in selecting a route. The negotiations for a route were finally completed on 6 November 1903 as a result of the Panamanian separation from Colombia. Thereafter the Hay-Bunau-Varilla treaty was hastily written and ratified by the Senate on 23

51 Rodman, W. L. Correspondence: A voluntary national examining board. *J. Amer. med. Ass.*, 1902, *38*, 1525.

52 Proceedings of the house of delegates. *J. Amer. med. Ass.*, 1902, *38*, 1659.

53 Report of the committee on legislation. *J. Amer. med. Ass.*, 1903, *40*, 1375.

54 *United States statutes at large.* Washington, U. S. Government Printing Office, 1902. Vol. XXXII, pt. 1, Chap. 1302.

February 1904. In this manner the United States inherited a venture which had twice ended in the failure and bankruptcy of French developers.[55]

While the extravagance and graft of the French doubtless contributed to their failures, the fact remains that yellow fever would have brought the project to a halt even if the financial arrangements had been proper. This fact seems to have been recognized, but not properly appreciated, by an earlier Commission appointed by the Congress to compare the two routes.

> Perhaps the greatest difficulty to be encountered in the construction of the canal will be the procurement of an adequate force of laborers and the preservation of their health and efficiency. In this respect the Panama route has a lugubrious history from which the Nicaragua route is free. The notorious mortality which attended the construction of the Panama Railroad and later the operations of the Panama Canal Company has taught a lesson which will not soon be forgotten for that route.[56]

But forget it they did! When the Commission sent out survey parties in 1899 to make a comprehensive study of the alternate routes, they were instructed to return with detailed engineering and economic reports. Because there was no mention of health or sanitation in those instructions, it is not surprising that the 175-page report contains only one page on medical affairs and it concludes that a well-organized, efficient sanitary department with adequate authority would be required.[57,58]

The magnitude and importance of the task which was to confront the medical officers is indicated in the Commission's own report:

> It is stated by Mr. Banau-Varilla, at one time chief engineer of the old Panama Canal Company, that out of one hundred individuals sent to the Isthmus not more than twenty, as an average, could remain there, and even these lost a part of their value.[59]

Thus, the employment program of the French companies was less than twenty per cent efficient. At that rate no private company could have built the Canal when burdened with the cost of recruiting a labor force to be sent at great expense across an ocean, eighty per cent of whom would die there or require transportation

55 Miner, D. C. *The fight for the Panama route.* New York, Columbia University Press, 1940, pp. 371-84.

56 Report of the Isthmian Canal Commission: 1899-1901. *U. S. Senate Documents,* 58th Congress, 2d Session, 1905, no. 222, p. 114.

57 Preliminary report of the Isthmian Canal Commission: December 4, 1900. *U. S. Senate Documents,* 56th Congress, 2d Session, 1900, no. 5, p. 9.

58 *Op. cit.* (56), p. 114.

59 *Ibid.,* p. 114.

home. Economic questions aside, Gorgas knew that the Panama Canal would not be completed if the mortality rate from yellow fever became excessively high.

> We appreciated that, if the Americans were subject to this disease to any considerable extent, we should have great difficulty in keeping them at Panama, and in order to induce them to stay, we should have to increase wages to such an extent that the cost of the work would be very greatly increased. That even if we should find a white American force which would be willing to stay, and if we could afford to pay sufficiently high wages to induce them to stay, Congress, in all probability, would not sanction the continuance of the work, if we lost from yellow fever fifteen or sixteen hundred Americans every year. From the best statistics which I could get on the Isthmus, I found that the French lost yearly by death from yellow fever about one-third of their white force. If we lost in the same ratio it would give us about thirty-five hundred deaths among our Americans yearly.[60]

The failure of the Commission to appreciate the importance of controlling yellow fever as a prerequisite to the engineering of the Canal was perpetuated in the Act when it was passed in June 1902. Although it dwelt at length on various engineering problems, it contained not one mention of health or sanitation. Moreover, it required that four of seven members of the Isthmian Canal Commission be engineers, without recognizing that the appointment of a physician to the Isthmian Canal Commission was the best way to assure that the sanitary problems would be given adequate consideration.

The New York Academy of Medicine was the first group formally to call attention to the possibility of appointing a physician to the Isthmian Canal Commission. In December 1902 it passed a resolution reminding President Roosevelt of the magnitude of the medical problem.[61] The Editor of *The Journal of the A.M.A.* was quick to second the view that a physician be appointed to the Canal Commission and mentioned that Gorgas was well qualified because of his recent success in stamping out yellow fever in Havana, Cuba.[62] Gorgas, too, had seen the similarities between the Isthmian and Cuban situations. Early in 1902 he had written Surgeon-General Sternberg (a few months before the latter retired) and asked for permission to train himself for the command of the Sanitary Department at the Canal. Sternberg agreed and arranged for Gorgas to be sent to the First Egyptian Medical Congress to study the problems of building the Suez Canal and to Paris in

[60] Gorgas, W. C. *Sanitation in Panama.* New York, D. Appleton and Co., 1918, p. 149.
[61] General medical news. *J. Amer. med. Ass.*, 1903, *40*, 41.
[62] Editorial, *Ibid.*, p. 104.

1903 to examine the records of the *Compagnie Nouvelle* (the second Panama Canal Company).[63]

At the 1903 annual meeting of the A.M.A., Welch introduced a resolution, similar to that of the New York Academy of Medicine, calling on the President to appoint a physician to the Isthmian Canal Commission.[64] These efforts were supported by an A.M.A. National Legislative Auxiliary Committee that had been organized following a meeting of Welch, Rodman, and Reed in June of that year. By December the Auxiliary Committee gave the A.M.A. 1,940 local representatives for getting in touch with various legislators.[65] Its effectiveness was proved when the Annual Conference on National Legislation met in Washington in February 1904. The Conference urged the members of the Auxiliary to wire President Roosevelt requesting him to appoint Gorgas to the Isthmian Canal Commission. The response evoked resulted in the A.M.A.'s first big demonstration of political strength: fourteen hundred telegrams arrived at the White House within forty-eight hours.[66]

The Medical News, an independent, private medical journal applauded in an editorial:

> The Committee on Medical Legislation of the American Medical Association has indicated the keystone of the arch in the Panama Canal Commission. Surgeons, engineers, contractors, no matter how deserving of honor, nor how efficient in their work, will fall into what has long been called the "grave of the European" if they are not supported, nay more than that, controlled, by a medical officer who shall have full authority in carrying out the work of sanitation. . . . It is noted that Col. W. C. Gorgas of the U. S. Army and its expert in tropical diseases has been called in consultation with the members of the Commission and will in some measure have charge of these sanitary problems. They could be entrusted to no better hands. However, if the President rises to a full appreciation of the economic importance of health in its bearing upon the success of this prodigious undertaking, both during its construction and thereafter, he will not hesitate to clothe Gorgas with the dignity and authority of a full member of the Commission.[67]

However, the President did not grant the request of the A.M.A. The Commission was appointed without a physician. Yet this demonstration of organized strength so impressed Secretary of War William Howard Taft that he overruled his Army Chief-of-Staff and granted a request of the Conference on National Legislation that the Medical Corps have a seat on the Army General Staff.[68]

63 *Op. cit.* (60), pp. 138-9.
64 Official Minutes of the house of delegates. *J. Amer. med. Ass.*, 1903, *40*, 1377.
65 Report of the committee on medical legislation. *J. Amer. med. Ass.*, 1904, *42*, 1577.
66 *Ibid.*, pp. 1577-8.
67 Editorial, *Med. News*, 1904, *84*, 458-9.
68 *Op. cit.* (65), p. 1578.

In one respect Roosevelt did heed the advice of the A.M.A. when he told the newly appointed Commissioners to give special attention to the sanitation problem and intimated that they might find it best to give one man broad powers as head of the sanitary department.[69] But the Commission did not follow the President's wishes. Instead it chose to organize a Board of Health with almost no power, responsible to the Governor of the Canal Zone, who in turn was responsible to the full Commission in Washington. The Commission did not report directly to the President, but to the Secretary of War. The Commission selected Gorgas to be the Chairman of the Board of Health. Because no one down the line of command was given any real authority, each item recommended for purchase had to be approved by the Commission, weeks distant by boat. The Commission's ability to delay, to meddle with specifications, and to withhold appropriated funds led to inefficiency from an engineering point of view, but medically it led to fatalities. While deaths from yellow fever and malaria were on the increase, even quinine was being struck from the budget.[70]

Colonel Gorgas tells the story:

> An interesting incident occurred during this first year with regard to insect powder. Knowing that there would be some yellow fever to be dealt with in Colon and Panama, we estimated and made requisition for eight tons of insect powder. The reviewing authorities were very much shocked and surprised at the size of our requisition, and seized upon this one item of eight tons of insect powder to demonstrate the wildness of our estimates. It was some satisfaction to us for the Commission to see that we had not been wild and extravagant in our estimates, but that we had been obliged to use actually fifteen times as much as we had estimated for. . . . From the very beginning, the Commission underestimated the magnitude of the sanitary operations, as well as their cost, and when the sanitary authorities urged upon them more extensive preparation and larger expenditure, they thought us visionary and more or less lost confidence in us. This was very unfortunate both for the sanitary authorities, and for the Commission, and came very near being the cause of the complete collapse of sanitation.[71]

The fiscal arrangements under which Gorgas worked were designed for the minimum of efficiency. For Gorgas to order supplies it was necessary for him to call a meeting of the Board of Health, send a request to the Governor, who in turn sent it by boat to the Canal Commission in Washington. The Commission would then refer the request to its Subcommittee on Sanitation which was com-

69 Reed, Charles A. L. The Panama Canal mismanagement. *J. Amer. med. Ass.*, 1905, *44*, 812.

70 *Ibid.*, p. 812.

71 *Op. cit.* (60), pp. 151-2.

posed of two engineers, W. H. Burr and C. E. Grunsky. While these two gentlemen were probably highly competent in their own field, they were not competent to control a medical program. As one example of their misguided economies, they ordered the hospitals staffed with under-supervised interns and student nurses instead of offering the best medical care possible.[72]

In February 1905, Secretary of War Taft made a very curious appointment. He asked his fellow Ohioan, Charles A. L. Reed, who had become the Chairman of the A.M.A. Committee on Medical Legislation, to go to the Canal Zone as an impartial referee in a land value dispute.[73] Reed went to the Canal Zone and completed his assigned mission. However, the trip gave him an opportunity to inspect medical conditions in Panama. After witnessing the shocking manner in which the efforts of Gorgas were being frustrated, he wrote a report for *The Journal of the A.M.A.* As a courtesy he submitted a copy of the report to the Secretary of War a week before it was published. The following paragraph is typical of Reed's tone:

> The use of quinine as a preventative of infection did not commend itself to the great therapeutic skill or to the economic judgement of the Commission, more especially of Mr. Grunsky, so the observation of medical men with previous experience on the Isthmus, the conclusions of authorities in tropical medicine, such as Manson and Giles, the investigations of the Italian authorities and the unequivocal verdict of Koch was pushed aside and quinine for this purpose was stricken from the list.[74]

The ink was scarcely dry on *The Journal's* presses when the newspapers seized upon the charges. Gleefully, they retold the story of an *emergency effort* to purchase a nipple for a nursing bottle, a two-day process that Reed had reported step by step. He had even calculated the total cost of obtaining the authorizations required before purchasing the nipple. His sarcasm drubbed each point. He further charged that sections of the Canal Commission's own report were "absolute and unqualified falsehoods."[75]

The storm in the newspapers then began to rise faster than the yellow fever and malaria lists on the Isthmus. On 16 March the A.M.A. published the reply of the Commission which alleged that the tone of the Reed report constituted "sufficient evidence that it

[72] *Op. cit.* (69), p. 815.
[73] Report of committee on medical legislation. *J. Amer. med. Ass.*, 1905, *45*, 260.
[74] *Op. cit.* (73), p. 817.
[75] *Ibid.*, pp. 813-17.

is the product of a strangely biased mind."[76] But the newspapers as well as the doctors were not impressed with the Commission's detailed denial of many of the points raised by the Chairman of the Committee on Medical Legislation. To stem the tide of confusion and concern, Secretary of War Taft wrote the President stating that Reed's article was both "unauthorized and biased." On 21 March the Secretary's letter and the following reply from the President were released to the press:

I have received your report of the 17th, together with the report of Dr. Reed and the answer made thereto by the Commission. It appears from this that Dr. Reed's report (which of course should under no circumstances have been given to the public until you chose to give it, and until the answer thereto had been made by the Commission) was, without your knowledge printed in the Medical Journals.

It further appears that the statements which he thus published were unwarranted by the facts; and his accusations in many instances unsupported by proof.

Dr. Reed has not displayed in this report the qualities of temperament or the power of accurate judicial observation needed to make a report valuable to the Government. It is true that he was not charged with the duty of making such a report and that he was appointed to be a commissioner to assess value of real estate. Nevertheless when he assumed to make a report on sanitary conditions at your request as Secretary of War, he was under an obligation to speak with care and justice on so important a subject and to observe the proprieties as to its publication.

Judging from your report it appears that the chief difficulties that have arisen have come from the inherent faultiness of the law under which the Commission was appointed. It further appears, however, that in view of our experience with the workings of the Commission, a rearrangement of duties, and a change of personnel in view of this rearrangement, should be made. I am glad that you are shortly to submit to me a plan with these objects in view.[77]

Reed met the Presidential rebuke with a prompt apology, although he found it necessary to insist that he had been employed by the government as a referee, not as a medical consultant. His report on the sanitation issue was made as an agent of the A.M.A. and not the government. Secretary Taft had received an advance copy of the report only as a courtesy.[78]

The following comments which appeared in *The Lancet* (London), provide some degree of perspective on the Reed report:

There was much in this document, signed by so distinguished an authority in his profession, to excite disquiet and demand investigation.

[76] Walker, J. E., Burr, W. H., Harrod, B. M., and Grunsky, E. C. Reply of the Commission to the charges made in the report to the government by Dr. Charles A. L. Reed. *J. Amer. med. Ass.*, 1905, *44*, 1052-8.

[77] Morison, E. E., Ed. *The letters of Theodore Roosevelt.* Cambridge, Harvard, 1951. Vol. IV, pp. 1141-2.

[78] *Op. cit.* (73), p. 260.

Unfortunately, however, the report throughout was so disfigured by bias and often personal animus, and the circumstances of its unauthorized publication were so remarkable, even for America, as to deprive it of much of its weight.[79]

But the Editors of *The Lancet* admitted that the sensationalism of the Reed report was perhaps needed to point up the "false economies, delays in furnishing supplies, and the inherent faultiness of the law. . . ."[80]

The Medical Record gave Reed its approval in an editorial entitled "Dr. Reed's Indiscretion":

As physicians, we are not concerned, except in a general way, as all good citizens should be, with the ineptitude of the Panama Canal Commission, but we are very deeply concerned with the action of the Commission in thwarting Colonel Gorgas's efforts to save life on the Isthmus. If Dr. Reed's impropriety awakens the public mind to the imminent danger of failure of the all-essential work of sanitation of the Canal Zone . . . his indiscretion will be condoned and approved by those who have the wit to see that the canal will never be dug by dead men, or even by sick men.[81]

That the President had been disturbed by the rising toll of malaria and yellow fever and by the poor relations between Gorgas and the Commissioners is shown in a letter written more than a month before the publication of the Reed report. He reveals his impatience and his helplessness in dealing with the medical problem in a letter of 9 January to the Secretary of War:

It seems to me that Dr. Gorgas (or his successor) should receive a salary of $10,000 under the Isthmian Canal Commission and that his subordinates should be decently paid. I am wholly unable to understand why Admiral Walker should wish to leave untouched the useless and expensive New York office of the Panama Railroad, and yet should want to economize improperly, as it seems to me, in the medical service.[82]

The publication of the Reed report had brought all the President's doubts and the public's misgivings as to the operations of the Canal Commission to a head. By the end of March President Roosevelt was able to procure the resignations of the Commissioners. He replaced them with a new three-member Canal Commission that was to decentralize and streamline the Canal building operations and to improve sanitation. The Editor of *The Journal* congratulated the A.M.A. on its rôle in causing the reorganization of the Canal Commission:

79 Leigh, J. G. Sanitation and the Panama Canal. *Lancet*, 1905, *1*, 1531.
80 Editorial, *Lancet*, 1905, *1*, 1729.
81 Editorial, *Med. Rec.*, 1905, *67*, 500.
82 *Op. cit.* (77), p. 1095.

To what extent this action is to be attributed to the Reed charges may never be known; but according to the public press, they had much to do in justifying the President's action in the eyes of the people, to say nothing of its complete justification in the eyes of the medical profession.[83]

The new Commission gave Gorgas more authority and more money. But the process of reducing the incidence of disease was slow. Gorgas had set out to eliminate mosquitoes as the best means of controlling malaria and yellow fever. With the funds and manpower at his command, he chose to rid the Canal Zone of the standing water in which the mosquitoes bred, and to take care of other sanitation problems later. The Commission, however, did not see eye-to-eye with this choice. It became impatient with Dr. Gorgas for spending money to kill mosquitoes rather than remove the terrible stench from Panama City. One Commissioner proposed replacing Dr. Gorgas with an osteopath friend. Mrs. Gorgas in writing a biography of her husband called this incident a "grotesque joke" in which the man whom the A.M.A. had determined to be the most capable of fighting malaria and yellow fever was now to be replaced with a man who had "lived in the South" and had "seen yellow fever."[84]

After much deliberation, the Canal Commission did not choose the osteopath to be Dr. Gorgas's successor; instead it chose one Dr. Hamilton Wright, the son-in-law of Senator William Drew Washburn.[85] Wright did not place as much emphasis on the elimination of stagnant water as did Gorgas.[86] Secretary Taft seconded the Commission's recommendation and sent it on for the approval of the President. Roosevelt chose to consult Welch who, in addition to his other offices, was one of Reed's two colleagues on the A.M.A. Committee on Medical legislation. Welch replied that while he respected Wright, he would have to say that Gorgas was the man most qualified to conduct this work. President Roosevelt, impressed with the forthright tone of the Welch reply, wrote Admiral Walker as follows:

I enclose you a letter from Dr. Welch. It gives exactly what he thinks of Dr. Wright, and I wish to heaven we could get from other people, whose opinions we desire, such exact and straightforward information as this letter contains.[87]

83 Editorial. *J. Amer. med. Ass.*, 1905, *44*, 1120.

84 Gorgas, Marie D. and Hendrick, B. J. *William Crawford Gorgas; his life and work.* New York, Doubleday, Page and Co., 1924, p. 197.

85 Obituary: Hamilton Wright M.D. *Brit. med. J.*, 1917, *1*, 470-1.

86 Wright, Hamilton. *The malarial fevers of British Malaya.* Vol. I, No. 1 of *Studies from the Institute for Medical Research, Federated Malay States.* Singapore, Kelly and Walsh Ltd., 1901, pp. 16-20.

87 *Op. cit.* (77), p. 738.

Thus the President was advised by his Secretary of War and the Canal Commission to discharge Gorgas and by Welch to retain him. It was one of his old friends and hunting companions, Dr. Alexander Lambert, who helped him to make a final decision. He told Roosevelt that he must choose between "failure with mosquitoes and success without."[88] The President then overruled the Commission and ordered it to co-operate with Gorgas. With this new grant of authority the great physician was finally given the administrative backing to enable him successfully to reduce the mosquito population of the Isthmus.[89] That malaria and yellow fever were well controlled by 1907 and that the Panama Canal was opened in 1914 is history in which the A.M.A.'s legislative activities played a decisive part and constitutes one of the greatest achievements of the medical lobby. It is perhaps symbolic that in 1908 Gorgas was finally appointed to the Canal Commission and honored by election to the Presidency of the A.M.A.[90]

The success in dealing with the Canal issue left the A.M.A. Committee on Medical Legislation riding at its highest point since its inception. The Auxiliary Committee had grown to 2,800 members by the summer of 1905. The Auxiliary had been used *en masse* to send telegrams to the President in the effort to place Gorgas on the Canal Commission. More often, however, it was used to reach local political leaders; after these interviews, the Auxiliary Committeemen were asked to send letters of advice to the Committee on Medical Legislation. An effort was always made to strengthen rather than subordinate the local Auxiliary Committeemen. This stemmed from Reed's belief that the political success of the A.M.A. was dependent on grass-roots organizations. As one sign of its increasing political skill, the Auxiliary in 1905 prepared for the first time a directory of the local leaders of every recognized political part in the United States. It covered 900 counties and contained 11,000 names. The Committee on Medical Legislation said of the directory, "It thus happens that we are able to move with a certain degree of accuracy in invoking political influence. . . ."[91]

At this point the A.M.A. began to consider asking the Congress to grant it a national charter. Besides the obvious prestige and political advantages which might result from such a move, national

88 *Op. cit.* (84), p. 201.
89 *Op. cit.* (60), pp. 156-8.
90 *Op. cit.* (84), pp. 213, 268.
91 *Op. cit.* (73), p. 259.

incorporation would remove certain objections as to the legality of decisions made by A.M.A. conventions outside the boundaries of Illinois, where the A.M.A. had been originally incorporated. Late in 1904 a bill (H.R. 17335, 58th Congress, 2d Session) was introduced to give the A.M.A. the power to transact business anywhere in the United States.[92] After the House Committee on the Judiciary had consulted several experts, it decided that the bill was unconstitutional and amended it to give the A.M.A. corporate status only in the District of Columbia.[93] Doctors Welch and John S. Fulton were named to a special Committee of the A.M.A. to consider the question. In January 1906 they recommended that the matter be dropped because the Association would have no more advantages with a District of Columbia charter than it did with one issued by Illinois; in addition the A.M.A. owned real estate and a publishing plant in Chicago.[94] As a result there were no further attempts to press the Congress for national incorporation.

There were a number of unresolved issues before the Congress which attracted the attention of the Committee on Medical Legislation in 1905. There were antivivisection bills to fight. The Army needed a hospital near Washington which was later built with the legislative support of the A.M.A. Also, the Army was in need of a larger Medical Corps.[95] But the most significant issue was an attempt to pass a Pure Food and Drug Bill.

For the first time in its history the Committee on Medical Legislation was to be pitted against an older and previously more successful lobby. The foes of the Pure Food and Drug movement consisted of a coalition between the whiskey trusts, many of the food packers, the Retail Grocers Association, and the patent medicine interests. The patent medicine manufacturers had a powerful hold over public opinion because of their large volume of advertising in the daily newspapers. It is even reported that some of these manufacturers required the newspapers to solicit testimonials from local politicians. They paid from five dollars for a letter from an alderman to fifty dollars for one from a mayor. The newspapers could offer one additional inducement: "We'll give them all good send-offs, and publicity never hurts a politician."[96]

92 Bryant, J. D. Correspondence: Failure to secure incorporation of the association. *J. Amer. med. Ass.*, 1905, *44*, 805.

93 Proceedings of the National Legislative Council. *J. Amer. med. Ass.*, 1906, *46*, 212.

94 *Ibid.*, p. 216.

95 *Ibid.*, p. 210.

96 Bok, Edward. Editorial. *Ladies Home Journal*, April 1905, p. 20.

However, the Pure Food and Drug movement was helped by the national lay magazines which were not dependent on patent medicine advertisements. *The Nation* showed that the coercive power of the advertising dollar was so great that an Ohio newspaper publishers' organization threatened to print the name of the attending physician in every death notice should a Pure Food and Drug Act pass the Ohio Legislature.[97] As of September 1904 the following magazines refused to accept patent medicine advertisements: *Ladies Home Journal, Farm Journal, Saturday Evening Post, Good Housekeeping,* and *The Christian Herald.*[98] When *Collier's Magazine* defiantly announced on 8 July 1905 that it would accept no further patent medicine advertisements, a long article told the public why. Astutely, the Editor of *The Journal of the A.M.A.* told physicians that they should keep that issue of *Collier's* on their "waiting-room tables for some months along with subsequent issues."[99]

The public was told by the magazines how mass distribution of foods and increased chemical knowledge had combined to make hazardous the ever longer journey of foodstuffs from the rural producer to the urban consumer. To prevent spoiling, large amounts of poisonous chemicals were being dumped into canned foods. Dyestuffs were added to conceal the initially poor quality of the ingredients, while meat unfit for human consumption was often ground into sausages. Whiskeys were produced from alcohol by the addition of flavors and colors and then mislabelled to appear the natural product. The consumer was not only mislead and fleeced but, on many occasions, poisoned as well. A similar situation existed with regard to patent medicines; they were either ineffective or dangerous. Some, like Peruna, were alcoholic drinks. Others were opiates which caused addiction in adults and the death of children; morphine was sold as "Kopp's Baby's Friend—The King of the Baby Soothers."

One of the earliest and most influential of the magazines to aid in the fight was the *Ladies Home Journal* under the editorship of Edward Bok. He criticized the Women's Christian Temperance Union for not lending its support against the patent medicines which contained large quantities of alcohol.[100] He explained that ethical doctors do not patent drugs. His editorial page was used to

97 Editorial, *The Nation,* 9 Nov. 1905, *81,* 376.
98 *Op. cit.* (96), Sept. 1904, p. 17.
99 Editorial. *J. Amer. med. Ass.,* 1905, *45,* 198.
100 *Op. cit.* (96), May 1904, p. 18.

display the addictions caused by many of the nostrums. Most important, he challenged the theory of the *cure-all* and urged people to consult their own physicians for an individual diagnosis: "There is no evil in America today so great as this accursed passion for self-doctoring."[101]

The Ladies Home Journal exposed the shocking treatment of the letters sent in "sacred confidence" to the "institutes" operated by the patent medicine makers. It appeared that the ladies would describe their problems in great detail to the "doctors" who in turn would answer with a form letter which always concluded with the recommendation that the writer take the manufacturer's medicinal preparation. These letters, with names and addresses still attached, were subsequently sold to letter brokers who resold them for half-a-cent each through men's magazines under such fanciful titles as "55,000 Female Complaint Letters," "44,000 Bust-Developer Letters," and "The richest sort of reading you could possibly get hold of"![102]

In February 1906 Bok stated in an editorial that the medical profession could provide more help to the Pure Food and Drug movement than it had in the past:

> I would also ask the hundreds of physicians and medical associations who have written to this magazine to lend their attention and support to this legislation. With very few exceptions, physicians and medical bodies all throughout the country have been singularly lax and remiss to the "patent-medicine" measures which have come before the State Legislatures. Whatever may have been the causes for this remissness it is now distinctly the part of every physician, and the organization to which he belongs, to take an effective part in making this legislation possible and effective, so that legislators can no longer wonder at the absence of the medical profession at the hearings given these Bills.[103]

Bok also wrote a letter to the A.M.A. Committee on National Legislation reiterating the contents of this editorial.[104]

Although the Committee on National Legislation had declared that it favored a Pure Food and Drug Bill in 1904,[105] Bok's comments about lack of prior support from the medical profession were essentially correct. But the charge of remissness was incorrect. The A.M.A. had been pondering the problem for at least ten years. For the A.M.A. the problem was twofold; first, the physician needed to be educated in rational therapy; second, some organization was

101 *Op. cit.* (96), March 1905, p. 20.
102 Sullivan, Mark. Editorial. *Ladies Home Journal*, Jan. 1906, p. 18.
103 *Op. cit.* (96), Feb. 1906, p. 20.
104 *Op. cit.* (93), p. 215.
105 *Op. cit.* (65), p. 1578.

needed to examine various new and patented remedies. Many of these remedies were simply mixtures of various drugs which were often prescribed in combination; they saved the doctor the trouble of writing the amount and generic name of every component when he could write a prescription by brand name. However, in many cases the contents of the mixture were not known to the physician, in which case such a prescription became a public hazard. It was also apparent that standards were needed for products advertised in medical journals.[106] An important economic repercussion to the simple elimination of patents on drugs would have been the loss of funds invested by the pharmaceutical houses in research.

Finally, an attempt at solution of these problems was made on 3 February 1905 when the A.M.A. Board of Trustees voted to found the Council on Pharmacy and Chemistry. The Council was to set up standards for the quantitative and qualitative testing of drugs and to gather information on therapeutics. The results of the Council's labors were to be published in a book, *New and Non-Official Remedies,* admission to which was required of all products advertised in *The Journal of the A.M.A.*[107] The A.M.A. had gained an organization to evaluate drugs for its members and to control the activities of ethical pharmaceutical houses. By March 1905 it was ready to make a positive contribution to the cause of the Pure Food and Drug Bill by offering its advice to the Bureau of Chemistry of the Department of Agriculture, which was to enforce the bill if it should be passed. Furthermore, the A.M.A. was no longer to be in the hypocritical position of supporting a bill to regulate patent medicines while still carrying patent medicine advertisements in its own official publications.

But the ability of the medical lobby to perform was soon to be tested by the strongest lobbies then in Washington. The belligerency of the whisky trust on the Pure Food and Drug issue was typified by a statement of Mr. Warick H. Hough to the House Committee on Interstate and Foreign Commerce:

> I appear for the National Wholesale Liquor Dealers' Association of America, an organization which comprises the majority of the distillers, wholesale distillers, and rectifiers of the United States; and represents, it has been asserted, almost 90 per cent of the volume of business, a business from which the Government gets one-fourth of its entire annual income.[108]

106 *Op. cit.* (83), pp. 718-19.
107 Council on Pharmacy and Chemistry, *J. Amer. med. Ass.*, 1905, *44*, 720-1.
108 *Hearings before the Committee on Interstate and Foreign Commerce of the House of Representatives on H. R. 3044 et al.*, 59th Congress, 1st Session. Washington, U. S. Government Printing Office, 1906, p. 96.

In addition to the various vested interests which opposed the bill on the grounds that it would cost them profits, others were opposed to it because it would cost them power. Curiously enough, one of the men was William C. Woodburn, who within five years was to become the full-time representative of the A.M.A. in Washington. His opposition was on the purely personal ground that it would transfer some of his duties as District Health Officer to Dr. Wiley, the Chief of the Bureau of Chemistry. Also Woodburn chose to join with some Southern Congressmen in the contention that the bill infringed states' rights.[109] Many food manufacturers were worried that Wiley, who would be given the power to set standards of purity, would place too rigid an interpretation on the word "pure." It was charged that in the past Wiley had "placed an unjust stigma on the purity of canned vegetables."[110]

The editorial columns of *The Journal of the A.M.A.* told physicians of the need for grass-roots support if the Pure Food and Drug Bill was to be passed by the Congress:

> . . . the medical profession must join actively in the education of the public. With the commercial interests arrayed on both sides of the contest, the layman has small opportunity to glean the real truth from the mass of documentary ammunition (so-called 'literature') that is weekly being hurled from one camp to the other.

It also warned its readers about two magazines, *Public Opinion Magazine* and *What To Eat,* which contained inciting phrases such as "poisonous, nerve-racking, baby-killing food" and canned "factory refuse and street sweepings." *The Journal* contended that the publishers of these exaggerations and scientific untruths might be in league with the opponents of the Pure Food movement:

> To all appearances a most subtle attempt is being made to discredit the cause by a systematic series of attacks from the rear. Physicians, however, having well in mind the true merits of the case are in a position entirely to frustrate this scheme as they may come in contact with those who may read these exaggerated articles.[111]

In the summer of 1905 the A.M.A., the national magazines, the Federated Women's Clubs of America, the Consumers' League, and a newly formed National Pure Food and Drug League attempted to arouse public opinion in favor of the Pure Food and Drug Bill.[112,113] That summer the friends of the movement visited

109 *Ibid.,* pp. 53-5.
110 *Ibid.,* p. 91.
111 *Op. cit.* (99), p. 47.
112 Wiley, H. W. *The history of a crime against the food law.* Washington, H. W. Wiley, 1929, p. 52.
113 News item. *J. Amer. med. Ass.,* 1905, *45,* 50.

President Roosevelt and urged him to challenge Senator Nelson Aldrich and the "Old Guard" of the Republican Party on the Pure Food and Drug issue. These visitors to the President included Dr. Harvey W. Wiley (Chief of the Bureau of Chemistry in the Department of Agriculture, a member of the A.M.A. Council on Pharmacy, and chief individual protagonist of Pure Food and Drug legislation), Dr. Ira Remsen (President of the Johns Hopkins University and a friend of Welch), and Dr. Samuel W. Lambert (President Roosevelt's physician and personal friend). Another group visited the White House in November 1905 and secured the promise of the President that he would support the pending bill. That group included three State Food Commissioners, a Retail Grocers Association spokesman, a representative from a food canning company, and Dr. Reed, the Chairman of the A.M.A. Committee on Medical Legislation, who had so embarrassed the Administration over conditions at the Panama Canal.[114] The President made good his promise when he asked the Congress during his "State of the Union" message on 5 December 1905 to pass a Pure Food and Drug Act.

When the A.M.A. National Legislative Council, the successor to the Conference on National Legislation, met in mid-January 1906, it was invited to attend a ceremony at the White House. Later in the day the Council, headed by Reed, was addressed for the first time by a President of the United States: "I want to say just a word of greeting to you and to ask your influence on behalf of the Medical Corps, not only of the Army, but of the Navy." The President then asserted that too much praise could not be given to those medical men who had made possible the digging of the Panama Canal. He then went on to ask the continued support of the A.M.A. for the Army Medical Reorganization Bill.[115] These remarks by the President not only vindicated the Reed report, but also found the powerful Theodore Roosevelt asking the A.M.A. for legislative support; this was remarkable recognition for a seven-year-old lobby.

Although public opinion and the President were supporting the Bill, Senator Aldrich thought that the issue could again be allowed to die a quiet death in Committee. But the "Old Guard" had not planned on receiving an ultimatum from Reed and the A.M.A. Mark Sullivan, a reporter of the period, tells the story:

[114] Sullivan, Mark. *Our times: the United States, 1900-1925*. New York, C. Scribner's Sons, 1927. Vol. II, p. 530.

[115] Proceedings of the National Legislative Council. *J. Amer. med. Ass.*, 1906, *46*, 214.

About the middle of February 1906, the American Medical Association brought direct pressure on Aldrich. Doctor Charles A. L. Reed, of Cincinnati, Chairman of the Association's Legislative Council, told Aldrich he had back of him some 135,000 physicians, all organized locally into about 2,000 county units, each member instructed not only to act himself but to ask his patients and friends to bring pressure on the Senate. Doctor Reed told Aldrich further that he and the members of the American Medical Association were determined to carry this issue of the Pure Food Bill into partisan politics if necessary.[116]

The Senate responded almost immediately to Reed's ultimatum. On 26 February 1906 the Pure Food and Drug Bill passed the Senate sixty-three to four with Aldrich abstaining.[117] Edward B. Logan in his monograph on lobbying analyzed the situation as follows:

The work of the American Medical Association bore fruit and public feeling ran high throughout the country. Although there were other factors aiding in securing the passage of the bill in the Senate, it cannot be doubted that the influence exerted by the American Medical Association and its followers was chiefly responsible for winning over the Senators. The Senate had maintained its opposition to the measure for seventeen years and had it not been for the lobbying of the American Medical Association there is no telling how or when the lobbying on the other side would have been overcome.[118]

But even the victory in the Senate did not assure that the bill would become law: the Senate version had still to be approved by the House. The end of Congressional Session was drawing near. On 2 June the Editor of *The Journal of the A.M.A.* bemoaned the fact that the Pure Food and Drug Bill was stalled in the House and that passage seemed once again uncertain. He urged the members to arouse public opinion lest the bill be forgotten at the end of the session:

There is no organization to represent the people, except the American Medical Association, to bring pressure on those who have the matter in charge; but there are any number of organizations to represent those who are opposed to the bill.[119]

To help expedite matters, the Committee on National Legislation prepared a petition for presentation to the Congressmen. On 21 June, for example, seven of these A.M.A. petitions were entered in the *Congressional Record.*[120] The petitions and letters from constituents were to force the Congressmen to act on the bill.

116 *Ibid.*, p. 533.
117 *Ibid.*, p. 534.
118 *Op. cit.* (2), p. 7.
119 Editorial. *J. Amer. med. Ass.*, 1906, *46*, 1701.
120 *Congressional record.* 59th Congress, 1st Session, 1906, p. 8917.

When the question came to a vote on 23 June, the bill was passed by a large majority. Even many of the previous opponents of the bill voted for it, seeing that nothing but ill-will could result from their continued opposition. The only dissenters were seventeen Southern Democrats who feared that the bill violated the doctrine of "States' Rights."[121] On 30 June 1906 President Roosevelt signed the Pure Food and Drug Act into law.

The A.M.A. had won a great victory over one of the most powerful lobbies. The editor of *The Journal* explained the significance of the victory to the members of the medical profession:

> While giving credit to the few faithful members of the Senate and the House who year after year patiently worked for the measure, such as Senator Heyburn and Congressman Hepburn (and Congressman Mann toward the last—his final presentation of the case with the well-worked-out 'object lessons' was the most effective argument on the subject ever presented), there were certain forces outside of Congress which had much to do with its passage. Undoubtedly, the strongest and most effective of these was the medical profession.

Further, the Editor stressed the importance of the cohesive action on the part of the medical lobby:

> And the American Medical Association, acting for the medical profession, through its Committee on Medical Legislation, with its far-reaching state and county auxiliaries, made it possible for physicians all over the country to act in unison, and consequently with effect.[122]

In July Reed wrote a summary of the events of the preceding session of Congress. He said of the Pure Food and Drug Act: "This end has been reached largely because of the influence that you have actively exerted either directly or through your neighbors, on your representatives in both branches of Congress." His summary also noted that the A.M.A. Committee on Medical Legislation had successfully helped the Army Medical Reorganization bill through the Senate, but had been forced to watch it bypassed in the closing rush of business in the House. Reed reported that he had been in contact with the Speaker of the House of Representatives who promised that the bill would be given early attention in the next session. The most forward-looking section of the summary was the announcement that a thousand dollars had been appropriated to prepare a draft bill for a National Department of Public Health.[123] He had been able to report a successful year.

121 *Op. cit.* (114), p. 549.
122 Editorial. *J. Amer. med. Ass.*, 1906, *47*, 116.
123 Summary of legislative events. *J. Amer. med. Ass.*, 1906, *47*, 116.

The summer of 1906 marks the close of the first period in the development of a legislative lobby by the American Medical Association. It had been founded to oppose an antivivisection bill and to propose a National Department of Public Health. In seven years it developed from a three-member Committee on National Legislation into a powerful national organization having more than two thousand local representatives. In 1900 the A.M.A. and its Committee on National Legislation were so little known that the Committee felt obliged to explain to the Congress just whom it represented; by 1906 it had triumphed over Washington's most powerful lobby in order to secure the passage of a Pure Food and Drug Act.

The political significance of this development is that while the A.M.A. was uniting the medical profession of the United States on a national level, the lobby was developing its strength at the local level. The credit goes to Tuckerman for seeing that the A.M.A. should develop influence at the grass-roots level rather than through personal contacts at the Capital. It was this policy which enabled the A.M.A. to generate the required public opinion for the support of Gorgas and the passage of the Pure Food and Drug Act. Both Tuckerman and Reed knew that a Congressman was impressed most by the power of a vote. Under Chairman Reed, this knowledge was exercized in the development of the Auxiliary Committee and the National Political Directory as major instruments of political action. But, as we have seen, the lobby was more than once in need of personal influence, and on these occasions it was the respected Dr. Welch who possessed and employed that influence.

The Committee on Medical Legislation had been conceived to promote the public health and to advance the medical profession. The public health was promoted by the Marine-Hospital Service bill, the various military health bills, the Pure Food and Drug Act, and the support of Gorgas to the benefit of the workers building the Panama Canal. The lobby had done much to raise the status of physicians working for the Government. In 1905, the President of the A.M.A. observed:

> The disregard of the good work of the medical profession in the public service (to say nothing of the indignities offered) on the part of Congress is notorious; and it is only by such concentrated power of the profession, intelligently directed, that such injustice may be overcome.[124]

[124] McMurtry, L. S. Presidential address. *J. Amer. med. Ass.*, 1905, *45*, 148.

Chairman Reed of the Committee on Medical Legislation viewed status as more than a matter of vanity, but as a form of economic class struggle. At the beginning of the twentieth century he foresaw the great scientific advances of the medical profession and wanted to assure that its social status would advance proportionally.

> It is to be remembered that this is an age characterized by the rapid evolution of social groups and classes; that these groups and classes are already jostling each other for place; and that, consequently, that group or class, or, more specifically, that profession must be vigilant and courageous that would guard its prerogatives and conserve its status in a democracy presumably founded on the principle of equality.[125]

Surely, some measure of the present high status of the medical profession may be traced to this social consciousness on the part of Reed and his successors.

Thus the physicians' lobby early began to fulfill two functions in our society. It represented the consensus of expert knowledge and opinion which in later years secured the passage of the Harrison Narcotic Act of 1914, the labeling of lye and other corrosive substances in 1924, and the stronger Federal Food, Drug, and Cosmetic Act of 1938, to name a few. It also acted as a special interest group. The former is essential for intelligent legislation on highly technical problems, while the latter is a requirement for social and economic advancement in a society where every individual has become a member of one or more special-interest groups. With these dual responsibilities, the medical lobby had become an important part of the American political milieu.

ACKNOWLEDGMENT

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[125] *Op. cit.* (73), p. 260.

Cutting for the Stone

HARRY S. SHELLEY*

CUTTING for the stone was probably the earliest elective operation practised in an attempt to relieve the human body of pain. It is interesting to speculate why this should be. Bladder stone was certainly more common in ancient and early modern man, probably due to a poor and unvaried diet, the resulting deficiencies of which we know can cause stone. Besides, bladder stones could be diagnosed with considerable accuracy without X-ray. The persistent severe pain of the bladder stone, as well as the fact that an appreciable number of those operated on for it survived, tended to make the sufferer willing to chance an operation. By our standards the mortality was high; but compared with the stupendous number of deaths resulting from attempts at abdominal and thoracic surgery it was relatively low, and so probably seemed both to doctor and suffering patient not too prohibitive.

Just who first passed a curved metal sound into the bladder and noted the metallic click that denotes the presence of a calculus we do not know. He must have been some ingenious ancient because the device is very old. Trepanation[1] and circumcision[2] are undoubtedly older than lithotomy; both of these, however, had a semi-religious background. Cutting for the stone is mentioned in the Hippocratic oath. Apparently the Egyptians did not do lithotomy for there is no reference to it in any of the known papyri or in any of the sculpture. It is thought that the Hindus knew and practised an operation similar to that of Celsus.

There were five general types of lithotomy: (i) Celsian method, apparatus minor or cutting on the gripe; (ii) Marian operation, or apparatus major; (iii) lateral operation; (iv) apparatus altus, or suprapubic cystotomy; (v) lithotomy by the rectum, or proctocystotomy. Each of these had many other names; and of the lateral operation there were many variations of the procedure and of the

* Nashville, Tennessee.

1 Trepanation was apparently performed by neolithic man, as skulls of that period have been found showing evidence of this operation. Sigerist, H. E. *A history of medicine.* New York, Oxford University Press, 1951, vol. I, p. 110.

2 It is not known whether the Egyptians learned of circumcision from the Jews or vice versa. The Egyptian operation according to the bas-reliefs that have been found was performed at about puberty while the Jewish rite was done on the newborn.

incisions. Celsus[3] gives us the first clear description of the operation in *De re medicina,* written about 10 or 20 A.D. He described what is called the apparatus minor, so called because of the few instruments needed in it, in contradistinction to the apparatus major or Marian operation, where a greater number of instruments was needed.

Celsus advised that the apparatus minor be used only in young boys not older than fourteen years and that the operation be done in the spring time only. The age limit seems to have been adhered to throughout the following centuries except by charlatans and reckless surgeons, and when it was violated the results were almost invariably disastrous. Celsus advised that the procedure be done in the following manner. After a preparatory period of several days on a light or fasting diet the patient was given a clyster or enema to clean out the bowel and was instructed to walk and jump so the stone would descend to the neck of the bladder. When the time for the operation had arrived he was placed in the lap of a "strong and intelligent person" who held him in a jack-knife position exposing the perineum. To further immobilize him and compress the abdomen the holder pressed his breast against the patient's shoulder blades. If the patient was a large individual the single holder might be replaced by two persons with their legs tied together. Additional assistants and holders stood at the sides. The operator stood or sat facing the perineum and the forefinger of the left hand "well dipped in oil" was inserted in the anus and pressed upward and forward toward the pubis. The right hand was placed in the suprapubic region pushing on the bladder and forcing the stone into the grip of the left forefinger, which pulled it down so as to produce a perineal bulge. With the scalpel held in the right hand a slightly curved, transverse incision was made in front of the anus with the convexity directed toward the bulb of the urethra. The incision was carried deep to the stone, the bladder being cut in the trigone or just posterior to it. The stone was then pushed out by the left finger in the rectum. If the stone was large, a "crochet" or hook might be used; this was a rod-like instrument with a handle, the opposite end being beaten out thin and curved in a semicircle with the outer side smooth where it was in contact with the flesh and the inner surface roughened to grasp the stone. This instrument was passed into the bladder, hooked around the stone, and slowly and gently withdrawn with a side-to-side and forward

[3] Celsus, translation by G. F. A. Collier, 3d ed. London, Longman and Co., 1838, Book VII, chap. xxvi, para. 2, pp. 307-13.

motion. The wound was not sutured. If the hemorrhage was severe the patient was put in a bath of vinegar and salt. Two hours after the operation he was placed in a tub of hot water. Then the wound was dressed with wool and warm oil.

It can readily be surmised that if this operation were attempted on an adult with well-developed prostate and seminal vesicles it would be fraught with danger, as there would be very little chance of pulling the adult male bladder into the perineum, while in a boy before puberty it might be accomplished without irreparable damage. The average size of the prostate in a boy of five is 1.2 cm. long, 1.5 cm. wide, 0.9 cm. thick[4]; at puberty it starts to grow very rapidly so that in an adult in his twenties the same measurements are 3.3, 4.1, and 2.4 cm.,[5] respectively.

The Celsian operation was the only one performed for the next 1500 years; and the disastrous results from its use in the adult male may be one of the reasons why "cutting for the stone" fell within the sphere of the itinerant lithotomists and was dropped from the armamentarium of regular surgeons, either "short robe" or "long robe." Possibly these roaming "stone cutters" had developed an improved method which they kept as a family secret and passed from father to son. We do not know the method they followed in removing stones.

A new method of lithotomy appeared around 1520, devised by Francisco Romano of Cremona, improved and described by his student, Mariano Santo di Barletta in 1535,[6] after whom it has been named the Marian operation. It sprang from the observation that stones would pass fairly easily through the short, dilatable female urethra. If the male urethra could be made shorter and dilated, then stones could be more readily removed from the bladder. This was accomplished in the following manner as described by Laurence Heister in 1739.

The patient was placed on his back on a table with a back-rest so as to raise the head and chest. His legs were bent at the hips and his knees flexed so they were almost touching his chest, thus the perineum was brought into a nearly horizontal position. One assistant stood at each side and held the legs stationary and spread the thighs; another held the patient's chest to keep him on the table; a fourth stood at the patient's right side, held the scrotum,

4 Lowsley, O. S. and Kirwin, T. J. *Clinical urology*, 2d ed. Baltimore, Williams and Wilkins, 1944, vol. I, pp. 794 and 795.

5 *Ibid.*, p. 794.

6 Heister, Laurence. *A general system of surgery*, 2d English ed. London, Innys, Davis and Clarke, 1745, vol. II, p. 110.

spread the perineum, and held the catheter (staff or sound). The staff was then passed and the stone searched for; and if the diagnosis was confirmed, the operation proceeded. After all the curved portion of the staff was in the bladder, the staff was turned so the convex part pointed toward the left side of the perineum. The handle was then gently inclined to the patient's right groin and given to an assistant to hold. This elevated the perineum and urethra. A perpendicular incision about three inches long was made in the middle of the perineum near the median raphé and the urethra opened in the bulbous portion. A grooved staff was usually used. This groove, which served as a guide for the knife and the dilators, was on the side of the staff so as to make the cut in the urethra and the prostate on the patient's left and thus more convenient for a right-handed surgeon. (If there were special staffs for left-handed surgeons, I have been unable to find any mention of them.) Next the male blade of the ensiform conductor or dilator was passed by engaging the projecting beak at the end of the conductor in the groove of the staff; this guided the conductor into the bladder (Figs. 2 and 3 of Plate 1). The staff was then removed; the female blade was passed by fitting the sulcus on the end of it over the longitudinal ridge which ran the full length of the male blade and guided the female blade into the bladder; here it was stopped by the beak on the male blade. Now there were two levers hooked together within the bladder with handles protruding through the incised bulb of the urethra; and by spreading the handles the surgeon could dilate or, as was most likely, tear the prostatic urethra and bladder neck. Thereupon the forceps were introduced and the stone grasped and extracted. If the patient was lucky, no bladder mucosa was caught in the forceps. These old surgeons were well aware of this danger, and the books are replete with cautions and ways to prevent it. It was advised to turn the forceps with one blade downward toward the rectum and slowly open them so the stone would fall into the jaws, and thus lessen the possibility of catching the bladder mucosa.

The French surgeons used instead of the ensiform conductors a dull gorgeret with a beak on it similar to the one on the male blade. The beak served to guide the gorgeret into the bladder. After removing the staff they rotated the gorgeret and gradually advanced it, thus dilating the bladder neck. They then used the gorgeret to guide the forceps into the bladder.

Occasionally a tube was placed in the bladder through the perineal wound. Heister and other leading lithotomists advised

against it and recommended leaving the wound open so urine, blood, and fragments of stone could drain out. Bleeding was controlled by pressure and pledgets of lint dipped in rectified spirits of wine. Arteries might be tied with a hooked needle and thread and the whole covered with a dressing of linen and held in place with a T binder. Sometimes the thighs were tied together to promote healing. This whole procedure must of necessity have been shocking to the patient, considering that no anesthetic was used and no anti- or aseptic precautions taken. It is remarkable that there were any survivors.

The lateral operation evolved from the Marian operation. There developed many variations; in its essentials, however, it consisted of cutting the body of the prostate instead of "dilating it" and cutting the bladder neck and some of the bladder wall.

The brutality of the Marian operation was quite apparent. Pierre Franco[7] was one of the first who tried to improve it and make it less brutal. He lived in the first part of the sixteenth century, dying about 1561, and practised in Berne and Lausanne. In 1556 he published his first edition of a *Treatise on Hernias,* which includes a description of his method of cutting for the stone. From his detailed account we cannot be sure just what he cut; that is, was he the true inventor of the lateral operation? He does not mention cutting the prostate. But from the remainder of his description we must conclude that he did cut the prostate, the bladder neck, and part of the body of the bladder. Also, from some of the instruments he devised it is apparent that he did everything to prevent bruising and stretching these structures. His double lithotome with hidden blades could have but one use and that was to cut the prostate and bladder neck (Plate 2). His cutting forceps could cut and grasp, but, more important, the handles were so cleverly designed that the jaws could open without producing a corresponding widening of the handles, as happened in the usual type of grasping forceps. This was accomplished by a double curve of the handles which allowed them to interdigitate. Franco is also the first to mention a two-stage procedure.[8] In cases of very large stones or in a debilitated patient, in whom the procedure would be very shocking, at the first operation he cut and did some dilatation; then several days later he completed the dilatation and extracted the stone.

[7] Bail, Pierre. *Un chirurgien urologiste du XVI^e siècle, Pierre Franco.* Paris, Joure et Cie, 1932, pp. 61-2.

[8] *Ibid.,* p. 66.

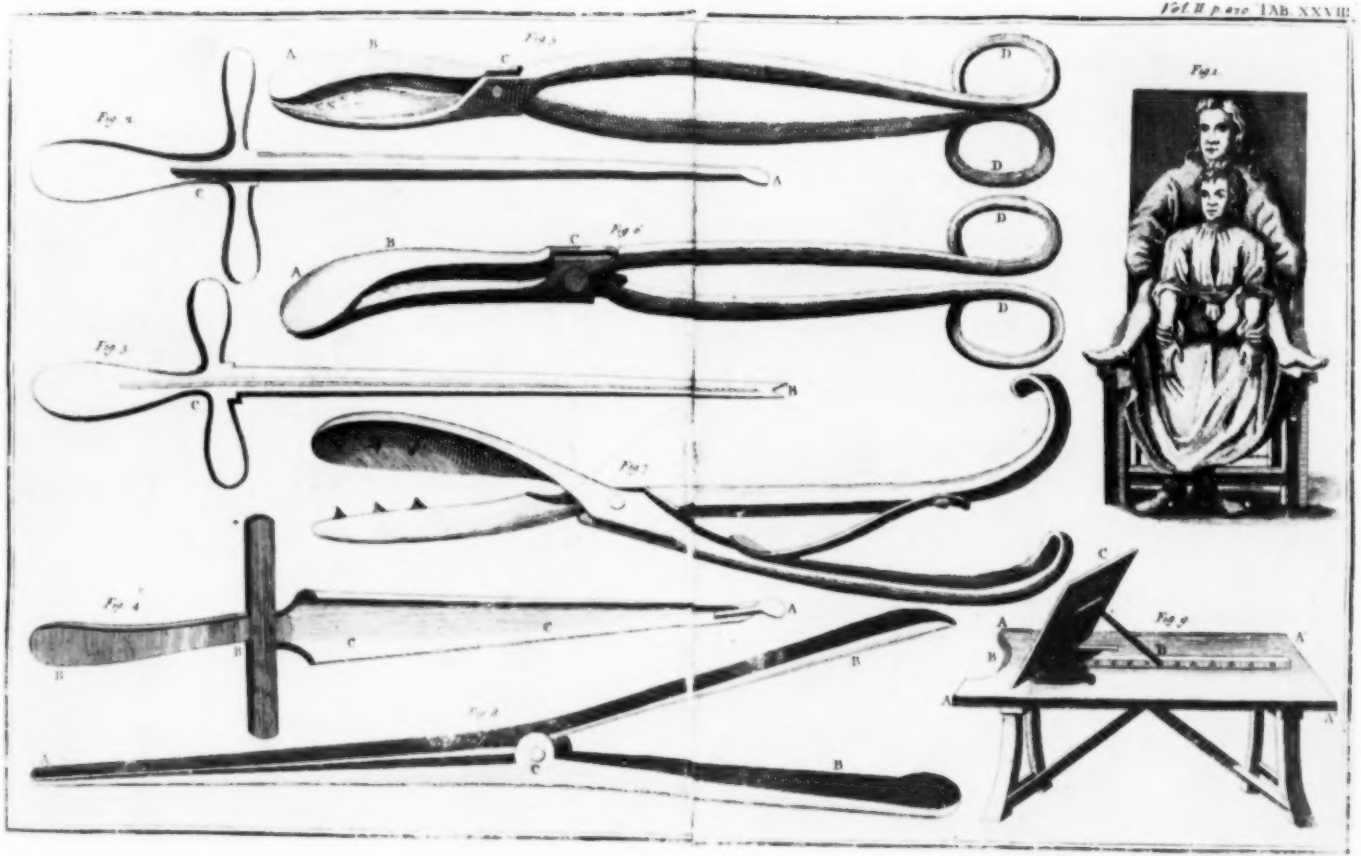


PLATE I

FIG. 1. Position of holding child for the Celsian operation. FIGS. 2 & 3. Ensiform conductors or dilators. Fig. 2 is furnished with beak A to fit in the groove of the staff and is called the male blade. Fig. 3 is called the female and Notch B fits over the ridge on 2. FIG. 4. Concave or cannulated conductor or gorgeret. FIGS. 5, 6 & 7. Stone extracting forceps. FIG. 8. Dilator. FIG. 9. Operating Table. Patient is rested at B, feet rest at A, and C support the patient's back. (From Heister's *System of Surgery*, 1743.)

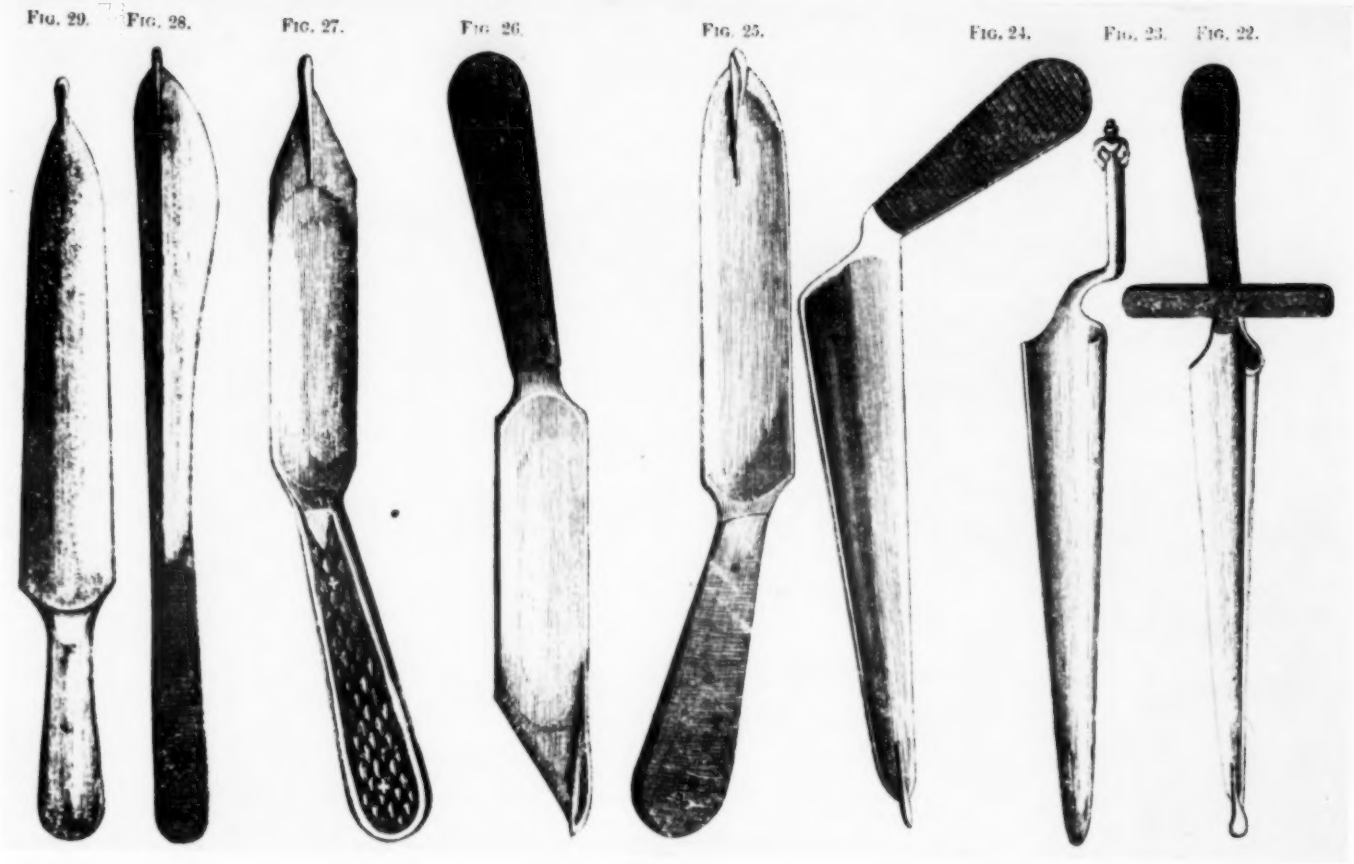
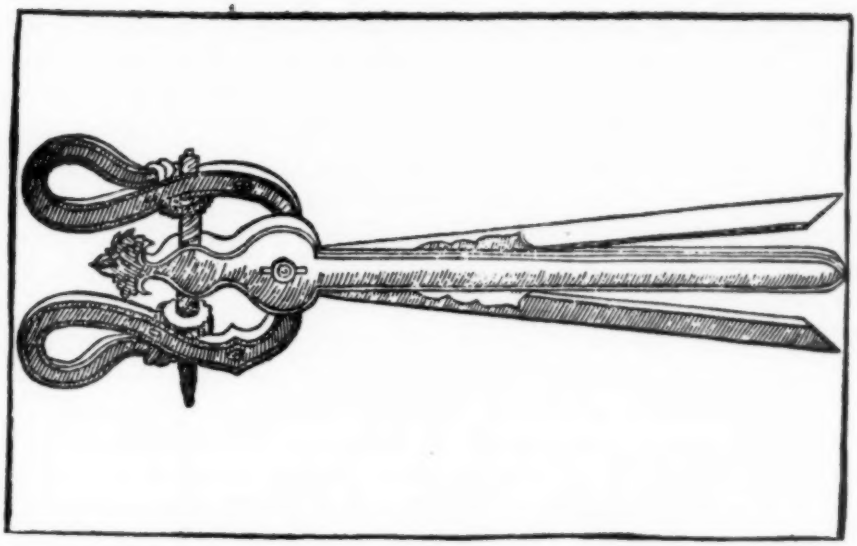


PLATE 4

FIGS. 22 & 23. Dilators of the urethra and bladder neck in the 'Apparatus Major' or Marion Operation. FIG. 24. Cheselden's dilator. FIG. 25. Cutting gorget of Hawkins. FIG. 26. Cutting gorget of Cline. FIG. 27. Cutting gorget of Sir A. Cooper. FIG. 28. Cutting gorget of Scarpa. FIG. 29. Cutting gorget of Keith. (From Thompson's *Practical Lithotomy and Lithotrity*, 1871.)



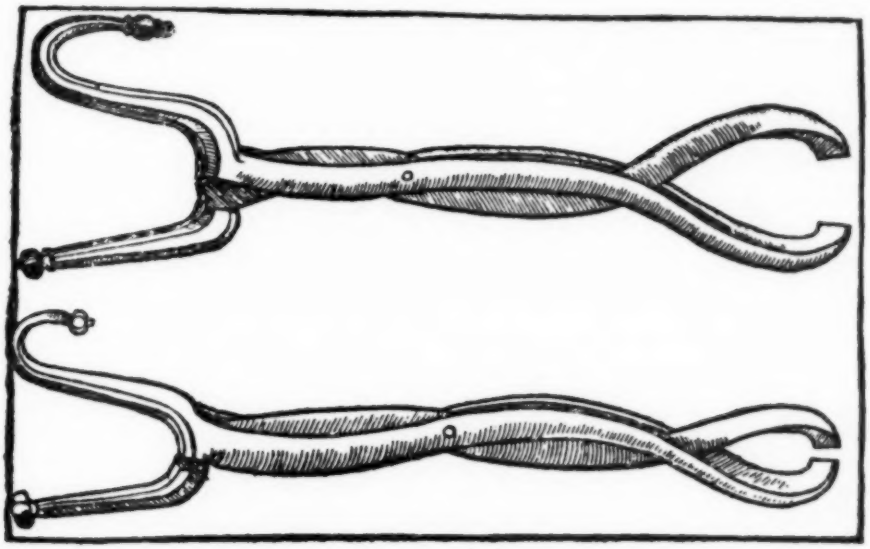


PLATE 2

(Above) Franco's double lithotome with concealed blades similar to one used in 19th century for the bilateral and medio-bilateral operations. (Below) Franco's forceps. The handles would not require more space than the stone which is grasped and the curved sharpened beak would prevent the stone from slipping.

In 1556 Franco reported the suprapubic cystotomy or apparatus altus or hypogastric resection.[9] This was for the removal of a stone the size of a hen's egg in the bladder of a two-year-old child. Although the operation was successful, he did not approve of the method and thought his satisfactory result was accidental.

The next lithotomist who will interest us is Jacques de Beaulieu (1651-1714), called Frère Jacques.[10] He was an untrained, uneducated pseudo-monk who had learned how to do the Marian operation from one of the wandering stone cutters, and devised an early type of lateral operation. In 1697 he appeared in Paris and offered to demonstrate his method and perform the operation. He refused any remuneration except food, clothing, and the repair of his instruments. He followed this policy throughout his life. His first demonstration in Paris was on a cadaver in whose bladder a stone had been placed. Subsequent study of the wound by the Parisian lithotomists, chiefly Méry, showed a skin incision about 4 centimeters in length a little to the left of the midline. It then passed between the bulbocavernosus and the corpus cavernosum muscles without cutting either, then through the body of the prostate, cutting the neck and part of the body of the bladder. Unfortunately, the first patient died, and investigation showed that the bladder had been torn from its attachments to the pelvis. After considerable wrangling he was allowed to continue his operation in Paris. He performed sixty, and twenty-five patients died. During the same period twenty-two operations were done by the older method with only three deaths. This concluded his Paris visit.

Bessière,[11] a Paris surgeon of the time who saw him operate on sixty cases, gave us this description:

> He laid his patient upon the operating table, and, placing a pillow under his head, gave him to the assistants to hold, with the thighs elevated and the heels bent towards the buttocks. He never tied his patients. He made use of a steel staff shorter from the handle to the heel, where it bends more than ours, and he had but two, one for women, one for men. He introduced this staff (big, round and having no groove) into the bladder, and, holding it with the left hand, he pressed it so against the perineum as to make that part of the bladder project which he meant to strike with the knife. Then, taking in his right hand a long knife, dagger shaped, he plunged it into the left hip at the distance of two inches from the perineum and pushing it directly onwards, opened the body of the bladder as near as possible to its neck, never once withdrawing the bistoury till he made an opening proportioned to the size of the stone, which he then introduced his finger to feel for, and running in a conductor along with the finger he

9 *Ibid.*, p. 69.
10 Kelly, H. A. Frère Jacques. *Aesculapian*, 1909, *1*, 145-51; Heister, pp. 158-61.
11 Kelly, p. 146.

introduced the forceps upon it, seized the stone, pulled it coarsely out, indifferent to the effects of that violence with which it was extracted . . . contents himself with a little oil and wine for a dressing and when once remonstrated with for his want of after care said, "I have extracted the stone, God will cure him."

Frère Jacques' failures were due to his ignorance of anatomy, which he soon realized and by diligent study corrected. He continued to do this operation with considerable success for the next twenty years, traveling all over Europe, and was offered many honors and rewards but declined them all. He is said to have operated on nearly five thousand patients. At one time he cut thirty-eight times without losing a patient. As his results improved, more surgeons took up his method. The most noteworthy was Johann Jackob Rau,[12] Professor of Anatomy at Leyden. Observing Frère Jacques when he was operating in Holland, and having a very precise knowledge of the structures, Rau was able to do the operation quickly and easily. He would never divulge his method; it is thought, nevertheless, that he cut from the bladder outward and incised the same structures that Frère Jacques did, but in the opposite direction.

Even if Pierre Franco[13] did a type of lateral operation, as is suggested by his writing, the method was not followed; and we must consider the operation of Frère Jacques as the connecting link or transition phase between the apparatus major and the true or nineteenth century lateral lithotomy.

In 1717 or 1718 James Douglass,[14] a skilled anatomist in London, gave a presentation before the Royal Society in which he demonstrated the anatomy of the bladder and its surrounding structures. This is the Douglass whose name has been given to the "pouch" (recto-uterine excavation). It is reasonable to assume that his brother John, a surgeon, studied this anatomical preparation and developed his suprapubic cystotomy as a result. John[15] performed his first high operation for the stone in 1719, and it was a success. In 1723 he published his results and mentioned that William Cheselden,[16] also of London, had used this operation eight times with satisfactory results. Cheselden was quite enthusiastic

12 Heister, pp. 158-61. Heister studied under Rau about 1705 in Amsterdam; from observing Rau operate on the living subject, Heister concluded that Rau cut from the bladder outward and cut on a large grooved sound.

13 Bail, pp. 62-6.

14 Cope, Sir Zachary. *William Cheselden*. London, Livingstone, 1953, p. 20.

15 Cheselden, William. *The anatomy of the human body*, 2d American ed. Boston, David West, 1806, p. 327.

16 Cope, pp. 19-30; Richardson, Sir B. W. *Disciples of Aesculapius*. London, Hutchinson and Co., 1901, vol. I, pp. 130-9.

about the suprapubic method for a short time. Later, several bad results due to injury to the peritoneum and intestines and the formation of fistulae caused him to become dissatisfied with the procedure, and knowing of Rau's success in Holland with the perineal route he devised a type of lateral operation. We are indebted to James Douglass, who saw Cheselden operate on numerous occasions, for a description of the method, for Cheselden left no clear description.

Cheselden, being an excellent anatomist, avoided all the difficulties of Frère Jacques and also introduced several new features. He used a hollow grooved staff and had his assistant hold the concave side snugly against the symphysis pubis to gain more space in front of the rectum and aid in its protection. He also filled the bladder with water, a trick he learned from doing the high operation. Placing the patient in the usual position, he made his incision, starting about one inch above the anus near the median raphé and carrying it downward and outward to the patient's left, he terminated it 2½ to 4 inches to the left of the anus. The left forefinger was then placed in the wound on the medial side and kept there, acting as a retractor and also protecting the rectum. In this first operation he inserted the knife in the bulb of the urethra and carried the incision to the apex of the prostate and then upward and backward through the prostate into the bladder. He soon abandoned this method, as he felt that the bulb of the urethra was in the way and the groove in the staff was hard to find and that there was too much chance of damaging the rectum. He reversed his incision by cutting through the side of the bladder immediately above the prostate and placing the point of his knife in the groove of the sound, cutting forward through the bladder sphincter, the left lateral lobe of the prostate, and the prostatic urethra, ending this internal incision near the urogenital diaphragm just proximal to the bulb of the urethra. When he incised the bladder, the water rushed out, frequently washing the stone to the bladder wound. The gorgeret was then passed by placing its beak in the groove of the staff; the staff was removed. The gorgeret was removed after it had served as a guide for the grasping forceps. He could frequently perform the operation in less than one minute. If the stone was large, the opening could be increased by further incision or dilatation by gradual traction on the forceps. Bleeding vessels were tied and dry dressing applied to the wound. The healing time varied from one to three months; occasionally the patient was left with a permanent fistula.

As to Cheselden's results,[17] in a late edition of his *Anatomy* he states that of 213 patients operated on at St. Thomas's Hospital, 20 died. Several of them contracted smallpox in their convalescence and some apparently died from that. This is certainly not a bad record for operations done without benefit of anesthesia or any conception of sterile technique and on charity patients of that day and time. I think we must credit these results to Cheselden's anatomical knowledge and his operative dexterity.

Many "gadgets" were invented in the next one hundred and fifty years that the operation was used. An early one, and one that was used by some lithotomists all through the period, was the "lithotôme caché" of Frère Côme,[18] the name taken by Jean Baseilhac (1703-1781), son and grandson of master surgeons. He received a good education and training, subsequently entered a religious order, assumed the name Frère Jean de Saint Côme, and devoted his life to tending the poor. He also invented the *sonde à dard*, a sound with a hidden stylet for use in the high operation, which will be described later.

The *lithotôme caché* consisted of a slightly curved rod with a handle on one end and on the opposite end a beak which fitted into the groove of the staff. Concealed in a slot on the convex side of the rod was a long, narrow knife blade, curved to correspond to the curve of the rod. This knife was hinged to the rod near the handle end and was also equipped with a handle. Squeezing the handles together caused the knife to protrude from its slot. The amount of protrusion and consequently the depth of the cut in the bladder neck and prostate could be controlled by a thumb screw between the handles. The resulting cut was of a uniform depth throughout the full length of the incision (Plate 3a). The patient was placed in the lithotomy position and the external incision made in the usual manner; then instead of the gorgeret, the caché was passed, the staff removed, the handles squeezed, and the caché withdrawn, leaving a clean cut in the vesical neck and the prostate. The stone was then removed with the forceps.

There was considerable opposition to the use of this instrument, although some continued to use it (particularly in females) in spite of objections. The uniform depth of the cut was an objection raised by many lithotomists, as they desired a more shallow

17 Cheselden, p. 333.

18 Desnos, E. *Historie de l'urolgie*. Paris, Doin et Fils, 1914, pp. 166-9; Thompson, Sir Henry. *Practical lithotomy and lithotrity*. London, J. and A. Churchill, 1871, p. 38; Gross, S. D. *A practical treatise on the urinary organs*. Philadelphia, Blanchard and Lea, 1855, pp. 618-19.

cut deep in the wound at the bladder neck than in the left lateral lobe of the prostate. But it did overcome some of the common dangers of the operation—cutting the rectum or the seminal vesicals; having the bladder collapse on the knife point as the water rushed out, resulting in a second cut in the bladder, usually in the dome and extending into the peritoneal cavity. These accidents most frequently occurred in the hands of poorly trained and incapable surgeons, of which there were many. Claude Pouteau of Lyons in the latter part of the eighteenth century cut 120 patients

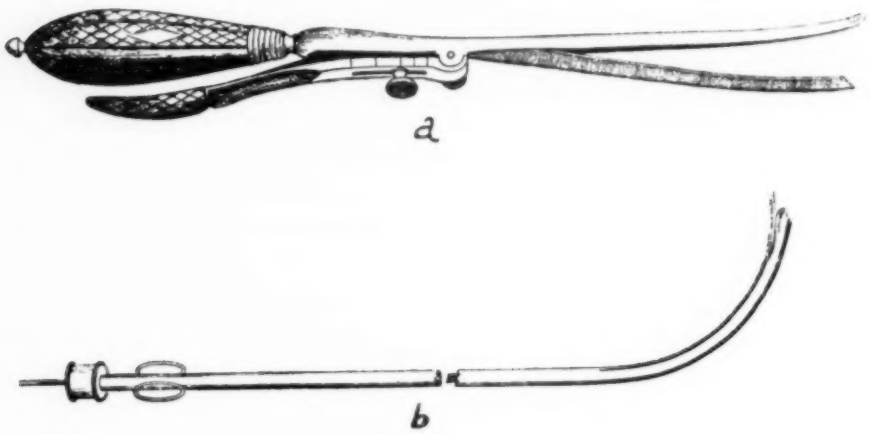


PLATE 3

Frère Come's (a) *Lithotôme Caché* showing blade open (late model).
(b) *Sonde à dard* showing the protruding stylet.

with it and lost only three—one of the best records I have been able to find.[19] The caché was never popular in this country.

Through these stages the apparatus major developed into the nineteenth century lateral operation. There was much discussion about the best method of incising the bladder neck and how deep to make the incision in the prostate. As described by S. D. Gross in 1855 and Sir Henry Thompson in 1871, the operation is very similar to Cheselden's first, except that they did not cut the bulb of the urethra but cut closer to the prostate, usually entering the urethra in the membranous portion and there locating the groove in the staff, or sound as it was called. They carried the incision back through the left lateral lobe of the prostate and bladder neck, being careful not to cut the prostate all the way through at its base. After the bladder was entered, the water rushed out, since

[19] Gross, p. 619.

the bladder was previously filled. Sound and knife were removed and the finger of the left hand slipped into the bladder to feel the stone and guide the forceps. Gross advised making the incision "as limited as it can be consistent with the safe and easy extraction of the foreign body." If it was too deep and went too wide, the seminal vesicals, rectum, even the peritoneum, and possibly the left ureter, were cut.

Deschamp, Chief Surgeon of La Charité, Paris, around the end of the eighteenth century gives a clear comprehensive description of the lateral operation as one

> making an incision obliquely from the raphé to the tuberosity of the ischium, then leading the cutting instrument between the ischio-cavernosus and bulbo-cavernosus to the groove of the staff; then, to cut through the lateral posterior portion of the membranous urethra, the transverse perinei muscle and a small part of the levator ani muscle, the whole extent of the neck of the bladder, the prostate and a little of the body of the bladder. This incision finished, there only remains to pass the forceps and grasp the stone.

The cutting gorget, an instrument that appeared in the mid-eighteenth century and produced a great deal of discussion as to its best shape and size, was an outgrowth of the old gorgeret. It varied greatly in its detailed design. The older instrument resembled a scoop or trowel, was used primarily as a dilator and guide for the stone forceps, and was passed into the bladder on the staff, being guided there by a beak or nib that fitted the groove. The newer instrument, while retaining the nib to keep it in the staff and guide the blade into the bladder, possessed a sharp knife edge on the forward end on either side of the nib. The blade was much narrower and flatter and of uniform width. The width of the cut in the prostate and bladder neck was controlled to a great extent by the width of the blade. Each lithotomist of any note designed a special gorget (Plate 4).

In the nineteenth century various types of skin and deep incisions were tried and had their followers. Some used a crescent skin incision with the concavity toward the anus and cut both lateral lobes of the prostate. Others used a median incision and cut the prostate in the midline posteriorly; this could easily lead to a cut in the rectum, especially in a young subject or one without any prostatic hypertrophy. Another type of median skin incision was followed by a short bilateral one into each lateral lobe. The lateral lithotomy was by far the most commonly used and apparently the most satisfactory (Plate 5).

The high operation, or the apparatus altus, was first tried as an experiment in Paris in 1475 by one of the Colots on a prisoner by permission of Louis XI. The man recovered in two weeks, but the procedure was not taken up by other surgeons.[20] The first detailed description we have is that of Pierre Franco[21] in 1556. Franco did not fill the bladder with water, but had his assistant

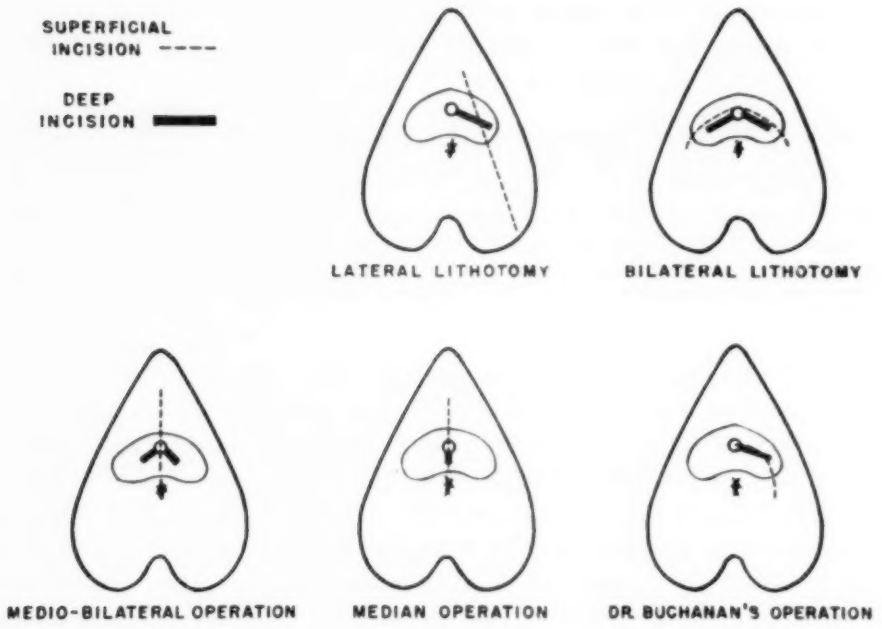


PLATE 5

Diagram of various incisions of the perineum in use in the 19th century, showing the outline of prostate and urethra and relation to anus.
(From Thompson's *Practical Lithotomy and Lithotrity,* 1871)

insert a finger in the rectum and press the stone forward; then he cut down on it in the suprapubic region, avoiding the peritoneum. This was in an infant of two years. The patient recovered. But Franco did not advise the use of this approach and used it very seldom. It was tried intermittently by various surgeons, who soon found that filling the bladder with water helped greatly. The general feeling until 1719, when John Douglass[22] revived it, was that it should be reserved for very large stones that would require

[20] Cooper, Samuel. *A dictionary of practical surgery,* from the 6th London ed. New York, J. & J. Harper, 1830, vol. II (under Lithotomy), pp. 137-9.
[21] Heister, pp. 126-49; Bail, pp. 69-73.
[22] Heister, pp. 129-30; Cope, p. 137.

considerable dilatation and would result in the tearing of tissue if removed by the perineal route. There was a renewal of interest in it in the early eighteenth century after Douglass and Cheselden tried it a number of times. It soon lost favor and was used only at rare intervals.

According to Cooper's[23] description, taken from Sharp's *Operations* (edition not given) the patient was laid on a square table with his legs hanging over the end and fastened to the table with a ligature above the knees, his head and body lifted a little by means of pillows so as to relax the abdominal muscles; the bladder was filled through a catheter with 8 to 12 ounces of barley water. The catheter was withdrawn and an assistant held the penis to prevent the water from leaking out, being careful not to stretch the skin of the abdomen. A four-inch incision was made in the midline between the recti muscles down to the bladder, care being taken not to injure the peritoneum. An incision was then made in the bladder and carried under the pubis. As soon as the water flowed out, the left index finger was hooked in the bladder to hold it open and protect the peritoneum. Forceps were inserted and the stone removed. Apparently Sharp[24] lost enthusiasm for the high operation; for in 1769, in his ninth edition, he wrote that it was little used and that considerable difficulty might be encountered in filling the bladder no matter how slowly it was done, and that it might be torn and the muscle tone be damaged by over-distending. Small stones were hard to find, drainage was poor, wounds frequently were slow to heal, and there was often much induration and infection before healing took place.

Around 1750 Frère Côme[25] introduced the *sonde à dard* to pierce the bladder (Plate 3b) and alleviate the discomfort of distending it. The *sonde à dard* was a hollow sound with a concealed stylet which could be pushed out of the tip of the sound by means of a rod that protruded from the handle end. With this instrument in place, which he had passed through a perineal urethrostomy, he made a suprapubic incision, pushed the peritoneum out of the way, found the tip of the sound through the bladder wall, and held the tip between the left thumb and forefinger, while an assistant pushed the stylet through the bladder wall. This small incision was enlarged by passing a bistoury into a groove in the stylet and then cutting downward almost to the bladder neck.

[23] Cooper, p. 137.
[24] Sharp, Samuel. *A treatise on the operations of surgery*, 9th ed. London, p. 119.
[25] Desnos, p. 181; Thompson, pp. 71-5.

The *sonde à dard* was removed and the stone extracted. At the end of the operation he passed a gum catheter into the bladder through the urethrostomy to provide drainage. This is the first use of a catheter following a lithotomy I have been able to find.

There remains one other method that was occasionally used; namely, lithotomy-rectovesical or proctocystotomy. It was proposed in the sixteenth century by an author named Vegetius,[26] but there is no record of its being done at that time. In 1816 a Frenchman, Sansom, used it in a few cases. Dupuytren used it for a short time. The patient was placed in the usual position, a staff passed and held perpendicular by an assistant. The lower part of the rectum and the sphincter ani were cut in the direction of the median raphé. This exposed the rectal surface of the prostate. The finger was passed in the rectum beyond the prostate until the bladder was reached, and the staff was palpated through the bladder wall. An incision about one inch long was then made in the bladder. At times the incision was carried into the prostate. The stone was then removed with forceps in the usual manner. In about eighty days, it is said, these wounds usually healed and the patient voided in the normal way without any fecal contamination. This method was used by only a few surgeons.

We of the present day may wonder why the high or suprapubic method, after having been used over four hundred years ago and having gone through several revivals, never gained a solid or permanent foothold until the last seventy-five years. Why were lithotripsy (crushing the stone) and lithotresis (drilling a hole in the stone) devised? Why were the *lithotôme caché* and all the various gorgets invented, when there existed to the modern mind the relatively simple suprapubic cystotomy? The answer, I believe, is three-fold: first, no relaxing anesthetic; second, no satisfactory ways or means of preventing infection; third, no satisfactory flexible catheter. Before the days of anesthesia, to reach into the bladder and remove a stone between two straining, tight, rectus abdominus muscles and struggle with a peritoneum being pushed down and out by a wide-awake patient would be no easy job. The danger of cutting the peritoneum would naturally be great; and in pre-Listerian days the chance of a fatal peritonitis can readily be appreciated. How much easier to cut through the perineum, where straining by the patient would cause little trouble and might help to push the stone out the incision.

[26] Cooper, pp. 153-4; Gross, pp. 627-9.

After the use of anesthesia, straining tight muscles ceased to be a problem. Anesthesia also allowed the bladder to be filled to a large volume, and thus gave more protection to the peritoneum by getting more of it out of the way of the wound. There still remained the trouble of having the hole in the water cask at the top and expecting the cask to empty itself by draining out the bottom. In fact, one author advised placing the patient on his stomach and keeping him there after the high operation. Another objection, and a valid one, was that when the bladder contracted, there was more resistance to the flow of urine through the normal channel than was offered by a hole in the dome and, as a result, the

GROSS'S TABLE

Methods	*Cases*	*Cures*	*Deaths*	*Proportion of Deaths*	*Mortality* rate per cent*
Apparatus major	1986	1580	406	1 in 4 7/8	20.34
Lateral operation	5418	4829	589	1 in 9 1/5	10.81
Bilateral method	207	175	32	1 in 6 15/32	15.45
Rectovesical resection	83	67	16	1 in 5 3/16	19.30
Suprapubic operation	180	141	39	1 in 4 8/13	21.66
TOTAL	7874	6792	1082	1 in 7 5/18	13.74

* (Mortality rates in per cent not given; author's calculations.)

urine would continue to flow out the suprapubic wound and delay healing for a long time. A good rubber catheter would have corrected all this difficulty. There were solid and flexible metal catheters, and later ones of gutta percha; but the articles which mention them also state that great difficulty was experienced in keeping them clean and free flowing.

In 1855 Dr. Samuel D. Gross[27] published some mortality studies on various types of lithotomy and gave the following table, which covers a period of about 150 years and includes such well-known lithotomists as Cheselden, Douglass, and Frère Côme and lesser dignitaries.

Gross also noted some exceptionally low mortality rates of individual lithotomists. Benjamin W. Dudley of Kentucky in the period around 1820 performed 207 lateral lithotomies and lost only 7—a mortality rate of 3.38 per cent. He used a gorget. Martineau, an Englishman, had one of the best hospital records—

27 Gross, p. 641.

2 deaths in 84 cases, or 2.37 per cent. He used a knife to cut the prostate. Pouteau, a Frenchman, using a lithotôme caché, lost 3 cases in 120, or 2.5 per cent.

Sir Henry Thompson[28] writing in 1871 compiled the following table of mortality rates and arranged it according to age periods. These were cases gathered from various hospitals scattered throughout the country and included both metropolitan and provincial areas. They are all lateral lithotomies of one type or

THOMPSON'S TABLE

During the ages of - inclusive	*Cases*	*Deaths*	*Proportion of Deaths*	*Mortality* rate per cent*
1 to 5 years	473	33	1 in 14 1/3	6.97
6 to 11 years	377	16	1 in 23 1/2	4.25
12 to 16 years	178	19	1 in 9 1/2	10.66
17 to 20 years	76	11	1 in 7	14.48
21 to 29 years	86	11	1 in 8	12.79
30 to 38 years	75	7	1 in 10 1/2	9.35
39 to 48 years	100	17	1 in 6	17.00
49 to 58 years	191	40	1 in 4 3/4	20.90
59 to 70 years	233	63	1 in 3 3/4	27.10
71 to 81 years	38	12	1 in 3 1/6	31.60
TOTAL	1827	229	1 in 8	12.50

* (Mortality rates in per cent not given; author's calculations.)

another (Thompson did not break the cases down according to type of procedure) done on male patients and performed around the middle of the nineteenth century; probably the majority were done under an anesthetic but not with antiseptic precautions.

This rate of 12.50 per cent for the middle nineteenth century does not compare well with Gross' 10.81 per cent for the period from 1700 to 1850. There is one circumstance that might have forced the rate up and that is lithotrity, which had gained considerable favor by 1870, while lithotomy was reserved for the worst cases (those with large stones). Thompson[29] says that at this time he was using lithotrity in five out of six of his own cases. He also collected the reports on 104 cases of the high operation, in which there were 31 fatalities, a mortality rate of 33.3 per cent. These were all very large stones.

28 Thompson, p. 142.
29 Thompson, p. 146.

It is interesting to note from Thompson's table that as late as the mid-nineteenth century most of the bladder stones occurred under the age of 21 years; of the 1,827 cases, 1,104, or 60.4 per cent, were under 21.

We have traced the development of the lateral lithotomy and noted its relatively low mortality rate and mentioned some of the outstanding surgeons who contributed to its development. Many prominent lithotomists, such as the Colots, Tolet, and le Cat, have been omitted since they were not primarily innovators. We have also seen the high operation come and go and return to stay, this return and general acceptance due surely to anesthesia, universal acceptance of Listerian antiseptic technique, and, finally, a satisfactory flexible rubber catheter.

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The Discovery of Insulin

W. R. FEASBY*

Introduction

STUDENTS of medical history a hundred years from now will probably be greatly puzzled by the varying accounts of the discovery of insulin which may come into their hands. It is astonishing to note the confusion already created about this event which lies so close to us in the recent past; 1921 is only thirty-seven years removed from today, and, with many of the men who were present at the time still living, one wonders how distorted and erroneous accounts arise. To the student of human nature, as well as the student of medical history, the reasons are obvious: the human observer is rarely completely accurate; the memory of man is a treacherous thing when unaided by proper records; and in some instances emotions and ambitions weigh and colour observations, omitting a fact here, emphasizing a phrase elsewhere, to impart some special meaning. Even the most judicial or scientific among us may be guilty of this at times, and especially when the accounts relate to a discovery so important as that made by Banting and Best in 1921.

In reviewing the accounts of this major discovery, it is easy to recognize all the possible sources of error. In addition, it is well known that the principals in this dramatic story had strong personalities and that, in the intensity of those days in 1921 and 1922 when a life-saving measure for millions was at stake, emotions and conflict of wills came into play. There is little doubt that the principals have all recorded their observations made at the time, and these will probably be made public when it seems appropriate to do so. In the meantime, an attempt should be made to keep the published facts straight and in their proper perspective, poor records should be noted as useless and misleading.

To a student of medical history writing in 1956, the full value of passing time is not yet available, but close contact with Banting, Best, and their colleagues and contemporaries has provided the

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present writer with insight and information not available to some of the other writers.[1]

Errors in Historical Accounts

Some of the accounts of the discovery of insulin have contained errors of fact; others have committed errors of omission. Some have attempted to minimize what Banting and Best did, although their names have now become household words in most parts of the world. Most of these stem from a facile but inadequate condensation of the events. Faulty accounts usually pass swiftly from discovery to the development of insulin, and vague generalities have done much harm. This was all most regrettable, and was regretted by none more than Banting and Best but was accepted and ignored during the heat of the struggle to save the lives of diabetics. Now perhaps, in view of certain publications, and with the advantage of documents and facts not previously available, it is time to correct some of the common errors.

Memorial Plaque

In 1955 the late Dr. Joseph H. Pratt published an account[2] which unfortunately contained errors of fact and several omissions and misinterpretations. He made reference to statements in a memorial plaque[3] as proof of his conviction that these men remained poorly informed of the real state of knowledge in their field of research. Banting and Best did not, in fact, write these words. When Banting was asked to submit suggestions for the plaque, he wrote in his own hand to the Board of Governors as follows: "In this room, in 1921, Banting and Best carried out the

[1] I knew Banting when I was a student in the University of Toronto and as a brother officer at the outset of World War II. I knew two of his close medical friends over many years and have learnt how to value his opinions and judgments of events. On several important points I treasure his own comments made in person to me.

Charles Best is better known to many of my generation because he has been our teacher, our counsellor, and friend for more than twenty years. Because of my interest in medical history, he has been especially helpful and has made all his personal and official records available to me. Those who know Charles Best will never doubt his ruthless honesty; perhaps it stems from years of brilliant and painstaking research, perhaps it is inherent in his English Maritime stock, but his meticulous and precise rendering of any fact, with all its relevant modifying factors, is well known.

[2] Pratt, J. H. A reappraisal of researches leading to the discovery of insulin. *J. Hist. Med.*, 1954, *9*, 281-9.

[3] "On the 30th of October, 1920, Frederick Grant Banting originated the hypothesis that the failure theretofore to isolate the internal secretion of the pancreas had been due to its destruction by the ferments liberated during the process of extraction.

"He devised an experimental method by which destruction could be avoided and the internal secretion (now known as insulin) obtained.

"In May, 1921, Banting and Charles Herbert Best, both graduates of the University of Toronto, conducted, in this room, the experiments which culminated in the isolation of insulin." (Room 221, Medical Building, University of Toronto.)

early experiments which led to the discovery of insulin. Yours respectfully, F. G. BANTING." Best was not a member of the mixed lay and medical committee responsible for the plaque and was not consulted by it. Dr. Best has repeatedly emphasized the vital importance in their research of Banting's working hypothesis which, as he has stated, was actually in large part responsible for the initiation of the Toronto experiments and for the use of several procedures which were vital for their success.

It is stated that there is a second error in the inscription on the insulin plaque, namely, that Banting was not the first to originate the hypothesis that the internal secretion of the pancreas might be obtained by tying the ducts. Banting *did* formulate this hypothesis *independently*, and the usefulness of a working hypothesis in the initiation or conduct of a research is not diminished by the fact that previous workers, using the same hypothesis, failed to attain their goal.

Preparation of Extracts

Certain other important facts were omitted from this account; for example, the insolubility of the active antidiabetic principle in 95 per cent alcohol, and in certain fat solvents, was reported by Banting and Best. It is important to quote their statement and the first paragraph of Collip's report, both of which Dr. Pratt omitted when he selected the latter part of Collip's paper for publication. These appeared in the *Transactions of the Royal Society of Canada* in 1922.[4] As they have not been readily available to many people, it is useful to publish them again here.

The Preparation of the Earlier Extracts
(F. G. Banting and C. H. Best)

In two previous papers a brief outline of the preparation of pancreatic extracts has been given. Active anti-diabetic extracts of degenerated gland, exhausted gland, foetal gland, and finally adult beef gland, were made. The main problem in the preparation was to get rid of or avoid the presence of proteolytic enzymes.

The first extract used was obtained by ligating the pancreatic ducts of the dog, and waiting from seven to ten weeks for degeneration of the acinar tissue. The remnant, which contained healthy insular tissue, was removed and macerated in ice-cold Ringer solution. By this procedure, a non-toxic extract which markedly reduced the blood sugar and the excretion of sugar in diabetic dogs was obtained in small quantity. Active extract was also prepared from the pancreas of a dog that had been injected with secretin.

[4] Banting, F. G., Best, C. H., Collip, J. B., and Macleod, J. J. R. Preliminary studies on the physiological effects of insulin. III. The preparation of pancreatic extracts containing insulin. *Proc. Trans. Roy Soc. Canada*, 1922, *16*, sect. V, 27-29.

The foetal calf extract was at first made by macerating pancreas of foetal calves of under four months' development in Ringer's solution, and filtering. Later, 95 per cent alcohol was used in place of Ringer's solution.

The alcoholic filtrate was evaporated to dryness in a warm air current and the resin-like residue redissolved in saline. This solution when injected subcutaneously or intravenously into a diabetic dog caused a marked fall in blood sugar and in sugar excreted in the urine. It was further found that this extract did not contain trypsin, that it was destroyed by boiling, that the active principal was insoluble in 95 per cent alcohol and that daily injections enabled a totally depancreatized dog to live a much longer time (70 days) than has hitherto been recorded after such an operation.

Potent extracts of the whole gland of the adult ox were obtained in a similar manner, using equal volumes of 95 per cent alcohol and pancreas, with the exception that the alcohol was made 0.2 per cent acid by the addition of HCl. It was found that the fatty substances in the extract could be removed by washing twice with toluol without deterioration of the potency of the extract. The alcohol could also be removed by distillation in vacuo at low temperature and it was found by reducing the volume to one-fifth instead of to dryness that a watery extract of the active principal was obtained. This could be sterilized by passing it through a Berkfeld filter. The extract in this form was given to a human diabetic and results in every way comparable to those obtained on the depancreatized dog were observed. However, owing to the high percentage of protein also present, sterile abscesses formed in a few instances at the site of injection.

The Preparation of the Extracts as used in the first Clinical Cases

(J. B. Collip)

The demonstration by Banting and Best that extracts of pancreas, prepared with certain precautions, contain a substance having the power to lower the blood sugar and to raise the sugar tolerance in diabetes, both in dogs and man, warranted an attempt to isolate this substance in a sufficiently pure state for repeated subcutaneous or intravenous administration in man. The problem was to remove most of the protein and salts and all of the lipoid material from the extracts without destroying the active principle. In the endeavour to solve this problem various methods were tried and the following was found to be most satisfactory.

In writing: "When Banting and Best state that they obtained 125 cc. of *tissue extract* from the pancreas of a dog 'whose pancreatic ducts had been ligated'," etc.,[5] Dr. Pratt has misquoted these authors. They wrote: "125 cc. of extract were prepared from the gland residue." Original notes show that they used copious amounts of chilled and sometimes acidulated saline. Admittedly, comparisons of potency using only one or two tests on diabetic dogs were premature and of no permanent value.

In support of his view Dr. Pratt refers to a letter by Roberts.[6] It is regrettable that Sir Henry Dale's reply[7] to Roberts' communi-

[5] Banting, F. G. and Best, C. H. The internal secretion of the pancreas. *J. lab. clin. Med.*, 1922, 7, 251-66 (p. 256).

[6] Roberts, Ff. Insulin. *Brit. med. J.*, 16 Dec. 1922, 1193-4.

[7] Dale, H. H. Insulin. *Brit. med. J.*, 23 Dec. 1922, 1241.

cation was omitted in an article reappraising the work of Banting and Best. The text is as follows:

Sir.—Dr. Ff. Roberts introduces his letter on insulin, published in your issue of December 16th (p. 1193), by the statement that it is "perhaps not out of place to review the steps which have led up to the production of this new remedy." Dr. Roberts's "review" takes the form of a destructive and somewhat censorious criticism, and its publication by him appears to me to be "out of place" for more than one reason. It is, of course, the right and the duty of every scientific worker to sift the evidence presented to him on any subject, and to accept only conclusions which he finds well founded. So far as the public exercise of our critical function is concerned, most of us find ample scope for its activity in dealing with the work of others in the field of our own investigations, where destruction may be necessary to clear the ground for our own building. Armchair criticism is, I believe, in general "out of place." It seldom leads to anything but verbal controversy. The proper antidote to bad evidence is good evidence, and not judicial censure. I doubt the right, and cannot understand the desire, of anyone to assume the rôle of independent critic, unless he has private knowledge of facts, which convince him that the scientific world is being misled by fraud, or the innocent public by pretentious humbug.

Now Dr. Roberts's letter is not aimed at anything of that kind. He takes the simple, honest record of experiments, which are doubtless not free from defects, and denounces them as "wrongly conceived, wrongly conducted, and wrongly interpreted." Dr. Roberts has no knowledge of the circumstances in which the work was done, though he could easily have obtained such knowledge. If he had done so, his letter would probably not have been written. He did not know that the work he attacks was the first, unaided attempt at research by two young enthusiasts; that one spent half the war as a combatant, and the rest, after being seriously wounded, as a medical officer in England, while the other has not even yet completed his student course. He had no conception of the personal sacrifice and heroic labour in which their enterprise involved them. Working thus on their own initiative, without the invaluable help and co-operation, given later by the head of the laboratory, who happened to be in Europe when the earlier work was done, they may have wandered along a wrong trail for a time, though this has yet to be proved. It may be that they made an unnecessary détour, before finding themselves at the point where E. L. Scott had stopped. The important point is that Scott did stop, and that Dr. Roberts would not be writing about his work now if Banting and Best and the other Toronto workers had not gone much further. I know that Dr. Scott would be the first to recognize this. It may be that the enthusiasm, which carried them further, was fired by an imperfect interpretation of their earlier results. If so, the mistake will be cleared up in time by others working more calmly and with more experience, and the truth will emerge.

There is nothing to prevent Dr. Roberts from putting his own theories to the test of experiment. Nobody can deny that a discovery of first-rate importance has been made, and, if it proves to have resulted from a stumble into the right road, where it crossed the course laid down by a faulty conception, surely the case is not unique in the history of science. The world could afford to exchange a whole library of criticism for one such pro-

ductive blunder, and it is a poor thing to attempt belittlement of a great achievement by scornful exposure of errors in its inception.

I am, etc.,
H. H. DALE.

December 18th.
London, N. W.

An editorial in the *British Medical Journal,* which appeared at the same time, may also be reproduced in part.[8]

The success of Banting and Best in obtaining, under laboratory conditions, an extract of the islands of Langerhans, and the effect of the injection of this extract on the blood sugar, has naturally attracted a great deal of attention. The account given in our columns by Professor Macleod (Nov. 4, p. 833), under whose superintendence the researches have been carried out in the physiological Laboratory of the University of Toronto, has been, of course, very carefully studied by physicians and physiologists. . . . The reports published by Banting and Best of their original method of preparing insulin, and of its modification and improvement, have been closely examined by physiologists in this country, and a certain number of criticisms have been heard. One expression of them is to be found in the letter from Dr. Ffrangcon Roberts, published in our issue last week (p. 1193). The points made by Dr. Roberts are of a technical kind, and the terms in which they were expressed, it must be admitted, were wanting in urbanity. Dr. H. H. Dale, who visited Toronto this autumn, has dealt with this aspect of the matter in a letter published elsewhere (p. 1241). While inviting Dr. Roberts to put his theories to the test, Dr. Dale states that no one can deny that a discovery of first rate importance has been made.

The points made in these articles about the early extracts and the fact that the working hypothesis was an indispensable springboard are those which should be stressed.

Dr. Pratt concludes his illustration of the failure of confirmation of Banting's productive hypothesis by saying that he apparently died without being aware of this. All that mattered to Banting was that he and Best had discovered insulin, that they could prepare a life-saving extract for diabetics, and that they had agreed it should be developed at top speed.

Banting and Best Purification of Insulin

Since, in 1921, in the opinion of many great authorities such as Professor J. J. R. Macleod[9] and Sir Henry Dale,[10] an internal secretion of the pancreas might not exist, or might be too labile to capture, Banting and Best could not, and did not, accept any of the various chemical properties ascribed to the hypothetical in-

[8] Editorial. The preparation of insulin. *Brit. med. J.*, 23 Dec. 1922, 1233-4.
[9] Macleod, J. J. R. *Physiology and biochemistry in modern medicine.* St. Louis, C. V. Mosby, 1918, p. 683.
[10] Dale, H. H. Advances in medicinal therapeutics. *Brit. med. J.*, 7 Jan. 1950, 1-7.

ternal secretion as facts on which they could base their work. They laboured under the conviction that insulin had not been discovered. In the light of this situation all the properties of insulin which they had demonstrated may be considered as their own discoveries. The fact that previous observers had used alcohol to extract a product, the existence of which the scientific world did not recognize, was not of great service to Banting and Best. They adopted a concentration of alcohol, now universally used, which they showed to be effective in extracting insulin and in precipitating the activated pancreatic enzymes which digest casein. They did not prove the presence of active enzymes in their extract, but *feared* that they *might* be present. They reported that active trypsin was not present in some of their extracts. They expressed the hope, in unpublished documents, that they could determine the exact range of the insolubility of insulin in alcohol.

The additional new procedures which Banting and Best used in the convincing demonstration and first purification of insulin were as follows:

1. The use of chilled extractive, chilled containers, and the maintenance of the original extracts solidly frozen until just before intravenous injection.

2. The use of 0.1 and 0.2 per cent of concentrated hydrochloric acid in saline, and later in alcohol, as an extractive of insulin.

3. Evidence that alkaline extracts were not potent when prepared under otherwise comparable conditions to those made acid.

4. The use of foetal pancreas, which is a very rich source of insulin, and from which a good yield can be obtained by very simple procedures. Banting and Best first used the chilled Ringer's solution technique, and later alcohol extraction of the foetal pancreas.

5. The demonstration that insulin was insoluble in fat solvents and in 95 per cent alcohol.

Perhaps the most significant contribution of Banting and Best was the record of their constant success in preparing, by simple procedures, very potent extracts from dog pancreas. These were not toxic when given intravenously. They were life-saving. Their notebooks contain the record of seventy-five instances in ten different depancreatized dogs when, without exception, their pancreatic extracts caused a very definite and, in some cases, dramatic and prolonged lowering of the blood sugar of diabetic dogs. This was frequently accompanied by such striking improvement in the well-being of the animals that no one could have doubted that a therapeutic principle for depancreatized dogs had been discovered. Banting and Best were convinced that no previous workers could

have observed the recovery of moribund diabetic dogs and the prolonged survival of animals without pancreas as they had done. If this had been so, they reasoned, surely hundreds of investigators would have been studying scores of different aspects of the action of insulin in diabetic animals, and tremendous efforts would have been made in university and commercial laboratories to secure a product for clinical use. There is, of course, no previous series of positive results comparable to those which Banting and Best obtained.

Dr. Pratt, not knowing all the facts, introduced many further errors of interpretation when he discussed the first clinical use of insulin.

Developments in December 1921

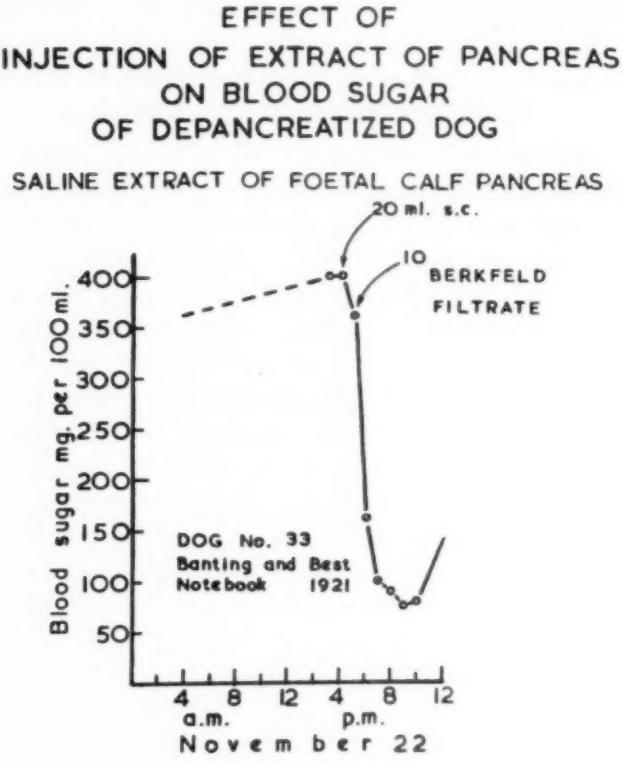
In October and November of 1921, Banting and Best found themselves on the horns of a dilemma. If there had been no obvious clinical application of their work, the horns would not have been nearly so uncomfortable. They would, quite naturally, have preferred to develop by themselves as many of the extensions of their findings as possible. They considered that their seventy-five separate demonstrations of the blood sugar lowering effects of insulin prepared from degenerated and intact dog pancreas, from foetal calf and from adult beef pancreas, when considered with the dramatic recovery of moribund diabetic animals, made the new phase of discovery complete. They had explored a number of avenues, as explained above, but they had no technical assistants, nor was any junior research worker assigned to work under them. They realized that unless the number of members of the team was increased, serious and inexcusable delays in many aspects of the work, and particularly in the clinical use of insulin, would result.

Professor Macleod did not "add a bio-chemist" to the group as Pratt states. The record indicates that this would not have been possible without the consent of Banting and Best. In December 1921, the further development of the purification was entrusted to Dr. J. B. Collip. Banting and Best discussed this move earnestly. Best was at first opposed, because he had not been given the opportunity, with adequate help and testing facilities on diabetic dogs, to do more than a few experiments on the chemical properties of insulin. The simple, relatively unpurified, but constantly potent extract was urgently needed, and its production in small lots was time-consuming. Banting argued: "This thing is far bigger than

we are, Charley, and the time has come to select certain problems for ourselves. Macleod and his associates will soon be anxious to get on with others." Banting was impressed with Collip's eagerness to help and insisted that all available chemical information must be given to him.

First Clinical Use of Insulin

But early in January 1922, Banting (who had decided to leave the experimental field for a time and to exercise his right to help



CURVE 1.

apply the insulin discoveries in the clinic) became very apprehensive that he and Best were being left out of the new developments. He went to Best and insisted that the latter should now make as rapidly as possible a new lot of the insulin preparation which they had used on Dog 33, and which had kept this depancreatized animal in good condition for ten weeks. Best suggested that an acid alcohol extract of foetal pancreas, which they knew to be very potent (Curve 1) should be used.

Banting was in favour of the extract of normal beef pancreas because it would be available sooner, and because, as he recorded at the time: "They will not be able to say that we did not prepare insulin from a commercially available source." Best removed, with aseptic precautions, the pancreas of a large steer which the abattoir authorities immobilized in a moderately convenient position. The pancreas was minced and ground with 95 per cent alcohol (0.1 or 0.2 per cent of concentrated HCl was consistently used in the alcohol), to give a concentration of 65 per cent alcohol. The mixture was filtered with the alcohol removed in a simple vacuum still. The aqueous solution of insulin was washed or extracted with fat solvents. The final product, after removal of the fat solvent, was tested for potency on a diabetic dog and was found to be "very active." (Banting gave subcutaneous injections of this material to himself and to Best. There was a mild local reaction.) This was the extract which Banting delivered to the Toronto General Hospital, and which was given to Leonard Thompson on 11 January 1922 and later to certain other patients.

In the situation which then prevailed, Banting found it intolerable that any insulin preparation other than the most successful one which he and Best had used on their dogs should be the first given to a human diabetic. This may have been an unwise move, but it is completely false to infer that Banting and Best had any opportunity to go as far as they could in purifying their product. The situation which existed really took this out of their hands.

This situation is described in Banting's own words as follows:[11]

> At this stage in the investigation, February 1922, Professor Macleod abandoned his work on anoxaemia and turned his whole laboratory staff on the investigation of the physiological properties of what is now known as insulin.
>
> Dr. Collip took up the biochemical purification of the active principle, and ran the scale of fractional precipitation 70 per cent to 95 per cent alcohol, and succeeded in obtaining a more improved end product. But, unfortunately, his method was not applicable to large scale production. Dr. Best then took up the large scale production and contributed greatly to the establishment of the principles of production and purification. This work was carried out in the Connaught Laboratories under Professor FitzGerald. . . .

Macleod discusses this phase as follows:[12]

[11] Banting, F. G. Diabetes and insulin. Nobel Lecture. Stockholm, 1925, 20 pp. Reprinted: *Canad. med. Ass. J.*, 1926, *16*, 221-32.

[12] Macleod, J. J. R. Insulin. *Physiol. Rev.*, 1924, *4*, 21-68 (p. 22).

They [Banting and Best], subsequently found that similar results could be obtained by using, as Zuelzer and E. L. Scott had done before,[13] extracts prepared from pancreas (of the ox) by means of alcohol. Not only were the hyperglycemia and glycosuria greatly reduced in degree by these products, but the animal improved strikingly in its general condition so long as the extracts were administered; one animal living for 70 days, when it was chloroformed for purposes of investigation. With the collaboration of J. B. Collip, (1922), the extracts were now fractionated by alcohol so as to rid them of materials which rendered them unsuitable for continued use on man because of local irritation, and more extended investigation was made of their effects on diabetic animals in the laboratory, and on diabetic patients. In this work, W. R. Campbell and A. A. Fletcher (1922) participated, and it was soon possible to show that every one of the symptoms of the disease could be completely removed by the use of the extracts.

Banting and Best wrote in May 1922 that their extracts from *whole* pancreatic glands produced local reactions when given subcutaneously, and that their method of preparation was being materially improved by various important modifications worked out by Dr. J. B. Collip.[14] In the first clinical report on insulin by Banting, Best, Collip, Campbell and Fletcher,[15] it is stated:

As the results obtained by Banting and Best led us to expect that potent extracts, suitable for administration to the human diabetic subject could be prepared, one of us (J. B. C.) took up the problem of the isolation of the active principle of the gland.

Professor Collip has more recently written:[16]

As a humble onlooker during the first few months when Banting and Best were carrying out the preliminary investigations which established the fact that insulin existed and that active extracts containing it could be prepared, I derived a great deal of satisfaction. . . .

In the early fall of 1921, I was invited by Professor Macleod, at Banting's request, to join in the work on the latter's problem. I shall forever owe a deep debt of gratitude for having thus been afforded the privilege of becoming a co-worker on this insulin project.

This is the true picture which has so often been distorted.

[13] A detailed discussion of the exact manner in which Zuelzer in 1908 and E. L. Scott in 1912 used alcohol is unnecessary here. It would appear that in Scott's experiments the wrong concentration of alcohol was used; the antidiabetic substance is insoluble, or nearly so, in the strong alcohol which he employed. Macleod's elision of the facts leaves the incorrect impression that these extracts were successful, and in this way he minimized, perhaps inadvertently, the importance of Banting's and Best's experiments which he goes on to describe.

[14] Banting, F. G., and Best, C. H. Pancreatic extracts. *J. Lab. clin. Med.*, 1922, *7*, 464-72 (p. 471).

[15] Banting, F. G., Best, C. H., Collip, J. B., Campbell, W. R., and Fletcher, A. A. Pancreatic extracts in the treatment of diabetes mellitus. *Canad. med. Ass. J.*, 1922, *12*, 141-6 (p. 142).

[16] Collip, J. B. Recollections of Sir Frederick Banting. Canad. med. Ass. J., 1942, *47*, 401-3.

Toxicity in Banting and Best Early Extracts

In the quotation of Dr. Frederick M. Allan, which Dr. Pratt publishes, Dr. Allan refers to the toxicity of pancreatic extracts which had been tried on animals before the work of Banting and Best. The extracts prepared by these workers in 1921 (and, Dr. Best tells me, by identical procedures today) were *not* toxic but life-saving. The foetal gland extracts which they made in 1921 were very potent. They could be sterilized and, if made available today, would give dramatic antidiabetic effects when given intravenously to diabetic dog or man.

Dr. Pratt misinterprets the statement about Zuelzer's extract made by Banting in his Cameron Prize Lecture.[17] Banting did not read German, and in the discussion of Zuelzer's work by others, the impression was given him that the toxicity of Zuelzer's extract may have been due to unrecognized hypoglycaemia. Banting did state that the first clinical results in Toronto, obtained with the extract which he and Best made, were not as encouraging as those reported to have been obtained by Zuelzer in 1908. He went on to say that subsequent results obtained with Zuelzer's extracts were completely disheartening, but that the results with his and Best's extract "were, however, sufficiently encouraging to change the whole trend of the research. Professor Macleod abandoned his work on anoxaemia and turned almost his whole laboratory staff onto the physiological activity of this pancreatic extract."

Minor Errors

Dr. Pratt's article also contains several minor errors.

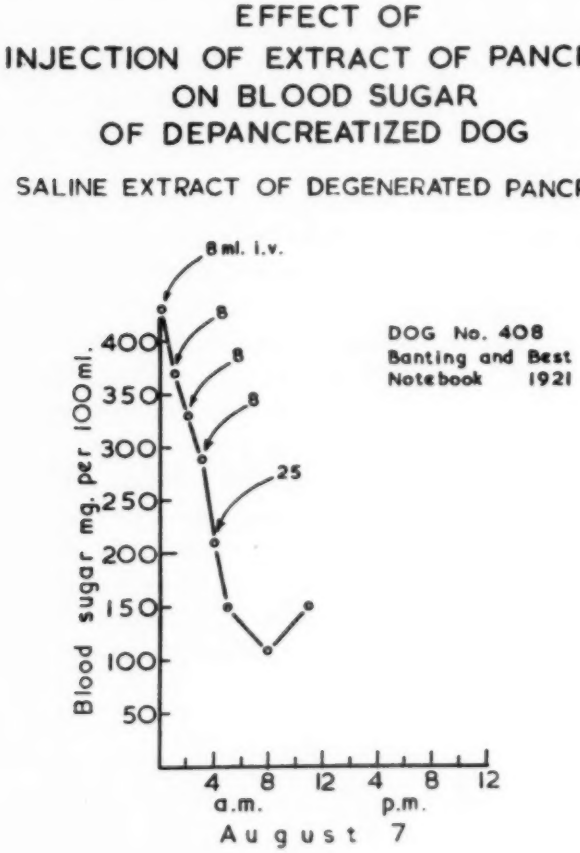
(i) Of Banting he says "a young surgeon" but this leaves out all the important points. He was 30, but he had served through four years of the Great War of 1914-1918, and was an experienced surgeon. Pratt omits that he had also had experience as a teacher and demonstrator in physiology.

(ii) Best he denotes as a "junior medical student" which he was not. He had just graduated from an Honour Science course at the University of Toronto. Pratt does not say that he had been a sergeant overseas in the Canadian Army and was thoroughly independent and mature at twenty-two.

(iii) Referring to the period in which they worked, he speaks of "many months," and later to "eight months." The work began

[17] Banting, F. G. The history of insulin. *Edinb. med. J.*, 1929, *36*, 1-18.

on 21 May, and the proof of the presence of a potent antidiabetic substance in the pancreas was complete by late August. Many verifications of its effect on numerous dogs had been completed by mid-September.

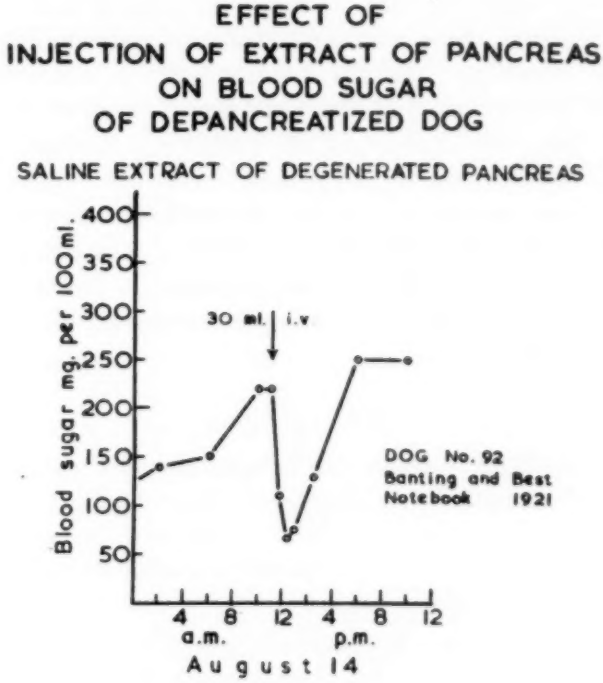


CURVE 2.

The early work on insulin developed very rapidly, but only certain points could be settled quickly by the two young investigators. The work, as I have just said, began on 21 May 1921. The first dose of chilled saline extract of degenerated gland was given on 30 July 1921, and this was quickly followed by others. A particularly dramatic fall in blood sugar was produced on 7 August 1921 (Curve 2).

On 14 August 1921, a deliberate attempt was made to see if the blood sugar could be lowered below the normal level of 0.1 per cent which was the normal fasting level with the methods then

used; 30 cc. of extract were given intravenously and the blood sugar level fell rapidly from 0.22 per cent to 0.66 per cent in one hour and ten minutes (Curve 3), and was only slightly above the normal level three hours and twenty minutes later. Elsewhere, in their notes, it is recorded that a dog with a considerably lower blood sugar rapidly improved when sugar was given.



CURVE 3.

The published and unpublished records of Banting's and Best's work establish the fact that convincing proof of the presence of insulin was available in the summer of 1921, when they were working alone and only on depancreatized dogs. The discovery of insulin was complete before any attempt was made to apply it to the human species.

(iv) Speaking of the hypothesis that Banting put forward (and which contained a misconception), Pratt uses the words "which Macleod shared with them . . ." This might give the impression that Macleod was present, but he was on holiday in Scotland until the end of September. Professor Macleod, who was very familiar with the biochemical and physiological literature, did acknowledge

(when the work was planned) that active trypsin might be present in certain extracts of pancreas.

(v) Pratt states: "They [Banting and Best] did not know that Heidenhain . . . had shown that extracts of fresh pancreas have no proteolytic activity." Banting and Best were acquainted with several of the papers to which Pratt refers[18] but they also knew that certain aqueous extracts of fresh pancreas may sometimes contain active trypsin. They were wrong, as events proved, in expecting that these extracts "must usually contain powerful proteolytic enzymes." In the original report on insulin, given before staff and students of the University Medical Faculty on 20 November 1921 and subsequently published,[19] Banting and Best stated that "the subsidiary hypothesis was that *trypsinogen* or its derivatives was antagonistic to the internal secretion of the gland." Trypsinogen and not trypsin was deliberately mentioned. Furthermore, Best records that, in the preparation of active pancreatic juice, he repeatedly made enterokinase preparations for the activation of the trypsinogen.

Relative Credit

In concluding his paper, Dr. Pratt assigned relative credit to the Toronto workers, but little significance can be attached to such estimates. The comments of those concerned, as contained in the foregoing pages, are clear and should eliminate the legends which circulate about this discovery.

Banting and Best appreciated fully the great contributions which Collip made in the development of insulin. His procedure, which has been referred to above, achieved a very considerable concentration and purification of their product and made possible for a time the convincing demonstration of an effect in the human species. It was not long before Collip encountered difficulties with this method, as Banting described in his Nobel Prize Lecture.[20]

Best was busy with a new research problem, but little time was given to it because in February 1922 when Collip left the group, Best[21] had to resume responsibility for the production of insulin. Some of the patients originally treated died in the few weeks before

18 It is perhaps regrettable that they did not make reference to Dr. Pratt's two papers on this phase.

19 See footnote 4.

20 See footnote 11.

21 Best had selected a study of the respiratory quotient before and after insulin in diabetic dogs (and in Joe Gilchrist, the first physician to receive insulin). This was the subject of his M. A. Thesis in the spring of 1922.

insulin became available again. Best's modifications consisted essentially in the use of acid acetone as an extractive and of a fine wind tunnel for rapid concentration of extracts. This made processing at a lower temperature possible and provided all the insulin used in Toronto for experimental and clinical work in the late winter, spring, and summer of 1922. Banting in his Cameron Prize Lecture states: "During the summer months of 1922, Mr. Best succeeded in producing quantities of insulin sufficiently large and sufficiently purified to prove thoroughly the value of insulin to diabetic patients."[22] These procedures were gradually modified with the help of the engineering staff of the Eli Lilly Company, and of the Connaught Laboratories until they were suitable for large-scale production.

Dr. Pratt correctly stated that Banting was the captain of the team. The captain assigned equal credit to Best for the discovery of insulin and none to those whose contributions were made during the development stage, e.g., after the repeated dramatic demonstration of the existence of insulin had fired the imaginations of all who saw or heard about these results.

Speaking at the opening of the Charles H. Best Institute in 1953, Sir Henry Dale summarized the whole story:[23]

> It was Macleod, also, who saw that, if Banting's attempt was to give any intelligible result he must have the co-operation of somebody with this recent biochemical training; and this recommendation was responsible for bringing Charles Best, recently graduated in Science, trained in the necessary biochemical methods, and himself rendered eager by a family contact with diabetes, to do something for those whom it afflicted, into the historic collaboration. Frederick Banting supplied, on his part, the determined, unquenchable initiative and an equipment with the necessary surgical technique. The collaboration was to be one of intimate understanding, with no question between the two participants of any but an equal sharing of its success. Matters having been thus arranged, Professor Macleod, still quite naturally sceptical of any successful outcome to the enterprise, left Toronto to spend the summer in Europe; so that it was in an otherwise deserted Department that the two young and inexperienced, but determined enthusiasts, working at tremendous pressure through the hot summer months of 1921, taking turns to sleep on occasion, solved the main problem without aid from, or communication with anybody. As a result, they had the clear evidence of the existence of insulin, and of the possibility of obtaining it in a separate solution, and of eliciting its effects by artificial injection, by the time Macleod returned from Europe.

[22] See footnote 17, pp. 9-10.
[23] Dale, H. H. Address. *Diabetes*, 1954, 3, 30-35 (p. 32).

SUMMARY

Facts relating to the discovery of insulin are presented in order to clarify the many errors and misconceptions of this event which have been circulated. Original documents, personal papers, and published comments by those principally concerned have been used. These establish the following points:

1. Banting and Best were aware of the many attempts to discover a pancreatic hormone; they believed these attempts were failures, which, indeed they were.

2. Banting and Best were provided with certain facilities for study by Macleod. Completely on their own, they discovered a method for extracting insulin from dog pancreas, adult and foetal beef pancreas, which would keep diabetic dogs alive. Evidence that the working hypothesis which motivated the work was incorrect, or that others had very nearly done the same thing, is irrelevant.

3. Banting and Best, using similar methods, prepared the first insulin used to treat a diabetic patient in Toronto.

4. J. B. Collip made important contributions in extending methods for purifying insulin, first recorded by Banting and Best.

5. The supply of insulin thus produced failed; Best was in charge of insulin production from mid-February 1922 at the Connaught Laboratories. There he developed a useful modification of the previous procedures and bridged the gap until others took up the task.

Notes and Events

Edited by Dorothy M. Schullian, Ph.D.*

Redi's Renal Calculi

The following extracts are translated from apparently unpublished originals in Baglivi's letter book (no. 7516) in the Osler Library. Both letters were written in Florence to Giorgio Baglivi in Rome in 1692 by Francesco Redi (1626-97), pioneer biologist and distinguished poet. He may have underestimated the age of his brilliant young correspondent, guessing he would reach his hundredth year in 1770, which figures, extra large and clear, are underlined. Baglivi, as it happened, did reach those figures but in tragic disorder, for it was in 1707 that he died at the early age of 38.

6 August, 1692 (leaf 109):

Since noticing that I have grown old, yes, seriously old, I make occasional slips in my writing, and believe me, my dear friend Signor Giorgio, it distresses me greatly. Recently I have been tormented by calculi and have given birth to Five of the hugest! *A Domino factum est istud* ["This is the Lord's doing"—the rest of the verse, "and it is marvelous in our eyes," being doubtless implied]. Patience! and moreover they have left me prostrated. Patience again, patience! You don't believe me; so I intend to come back and remind you of it in the year 1770, and then you'll get a fright. I jest because at the moment there is no pain, but when the calculi are racking me it is no laughing matter!

Da che io mi sono accorto di essere invecchiato, e invecchiato daddovero, commetto qualche mancamento involontario nello scrivere, E mi creda, caro am.mo sig.r Giorgio, che ne ho pena grandissima. A questi giorni ho tribolato di Calcoli, e ne ho partoriti Cinque grossissimi. A Domino factum est istud. Pazienza. E di più mi anno lasciato sconcertatissimo. Pazienza di nuovo; Pazienza. VS. non mi lo crede; Ed io lanno *1770* voglio venire e rammentarglilo, e VS allora avrà paura. Faccio il buffone, perche in questo punto non duole; ma quando i Calcoli fanno dolere, non faccio il Buffone.

27 September, 1692 (leaf 95):

By the good grace of the Blessed Deity I am at present in perfect, really perfect, health after passing seven small calculi which made their way down from the kidneys into the bladder. So now, in short, I am perfectly well, and am really and truly your Servant.

Per grazia di Dio Benedetto presentamente stò benissimo di sanità, e veramente benissimo, perche ho mandato fuora sette piccoli Calcoli che da' Reni mi erano scesi nella Vessica. Ora in sommo stò benissimo, e sono vero verissimo Servatore di VS. Ecc.ma . . .

W. W. Francis

* Cleveland, Ohio.

An Early View of the New York City Department of Health

On 20 June 1807, Baron Jean Guillaume Hyde de Neuville, a French royalist exiled for his refusal to swear fidelity to Napoleon, arrived in New York with his wife, the former Mlle. Rouillé de Marigny. They first traveled to Albany, next to Balston Spring, and then to the prime "must" of all early tourists in the United States, Niagara Falls. The winter of 1808 was spent in New York City, but in subsequent years they journeyed as far south and west as Tennessee. Returning to Europe with the news of Napoleon's downfall in 1814, Hyde de Neuville was rewarded for his loyalty to the Bourbons by an appointment as Envoy Extraordinary and Minister Plenipotentiary to the United States. From July, 1816, until June, 1822, except for one brief period, he and his wife were again in this country.

Throughout their American sojourns, while her husband discussed farming or politics or matters of state, the Baroness amused herself by making a series of drawings, beginning with one of the welcoming lighthouse in New York harbor in 1807 and ending with St. John's church in Washington in 1822. She was by no means a great artist, but her draftsmanship was good and her eye observant. Her works are charming depictions of both rural and urban scenes, full of human detail, and excellent source material for social historians. A few of them have been reproduced: they deserve to be more widely known.[1]

Of particular interest to medical historians is one of her scenes in New York, made in February, 1808. This sketch, which is now in the Stokes Collection of the New York Public Library and is here reproduced through the courtesy of the Library, is a wash drawing approximately 13 by 7½ inches in size. The large building in the center is the Bridewell, which was built in 1775-76 and demolished in 1838. To the left is the present City Hall, then in the process of construction, and in the foreground is the Charity School and its playground.[2] And down in the right-hand corner is the office of the Board of Health, along with a washerwoman hanging out her clothes, a cow, a dog, and an ugly looking hog wandering nonchalantly by, much in the manner described by Charles Dickens some thirty-five years later.[3]

The Board of Health in 1808 was a product of the series of yellow fever epidemics that had affected northern seaports between 1793 and 1805. Before that time the provincial government of New York had instituted quarantine measures as the occasion demanded until 1755, when a new law

1 The standard work on Hyde de Neuville is his *Mémoires et souvenirs*, 3 v., Paris, 1888-92. Brief notes on his American sojourns and reproductions of some of the Baroness's drawings may be found in I. N. Phelps Stokes and Daniel C. Haskell, *American historical prints, early views of American cities, etc. from the Phelps Stokes and other collections*, New York, 1933, 50-1; Frank Monaghan, "The American drawings of Baroness Hyde de Neuville," *Franco-American Review*, 1938, 2, 217-20; H. E. Keyes, "Early visions of the new republic," *Antiques*, 1931, *19*, 306-08.

2 Stokes and Haskell, *loc. cit.*, identify these and other landmarks in the picture.

3 Charles Dickens, *American notes and pictures from Italy*, London, 1893, 74-5 (first published 1842).

PL. 1. Office of the Board of Health, City of New York, in 1808. (Courtesy of Stokes Collection, New York Public Library)

Pl. 2. City of New York, Department of Health, in 1957. (Courtesy of the Department)

provided for a continuing quarantine system and a regularly appointed quarantine physician, the Health Officer of the Port. After New York City's epidemic of 1795, however, the state legislature at its next session in 1796 created a permanent Health Office Commission of seven members to administer the quarantine regulations and supervise a hospital established on Staten Island. In 1797 their number was reduced to four and in 1799 to three: the Health Officer of the Port, the Resident Physician, and the Health Commissioner, who was in fact an assistant to the Resident Physician. The law charged them with the superintendence of quarantine and also authorized them to make ordinances for the cleanliness of the city until 1805, when a new act transferred the latter power to the Common Council. This statute also authorized the Common Council to institute a Board of Health consisting of the Commissioners of the Health Office "and such other persons as they might think proper," and to invest in this Board its own powers relating to the protection of the public health. Under this act the Common Council designated the Mayor, Recorder, City Inspector, and five Aldermen, together with the Commissioners of the Health Office, as the Board of Health, and thus it remained with minor variations until the Baroness sketched this picture.[4]

The site of the health office in 1808, at the corner of Chambers Street and Broadway, is not far in distance from the present location of the New York City Department of Health at 125 Worth Street. But in size the office of 1808 is as far removed from the modern edifice as the simple quarantine and sanitary regulations of those days are from the present voluminous Sanitary Code. Comparison of the two buildings illustrates well the vast expansion of public health services in the last one hundred and fifty years.

JOHN B. BLAKE

Walter Charleton's NATURAL HISTORY OF THE PASSIONS *(1674) and J. F. Senault's* THE USE OF PASSIONS *(1649) A Case of Mistaken Identity*

Recent study of early English psychiatric literature led us to an anonymous book entitled *Natural History of the Passions* (London, 1674) (see Fig. 1). Although there is no hint of his name, its author was known to be Walter Charleton, M.D. (1619-1707), physician to Charles I, one of the first elected Fellows of the Royal Society, three times President of the Royal College of Physicians of London and three times its Harveian Orator. The

4 Susan W. Peabody, "Historical study of legislation regarding public health in the states of New York and Massachusetts," *J. infect. Dis.*, 1909, Supplement no. 4, 3-13; Sidney I. Pomerantz, *New York an American city 1783-1803: a study of urban life*, New York, 1938, 338-49; New York State, *Laws*, 1805, ch. 31; *Minutes of the Common Council of the City of New York 1784-1831*, 19 v., New York, 1917, 3, 712; 4, 43, 78, 107, 140, 312, 677; A law for the establishment of a Board of Health, Dec. 8, 1806, New York City Board of Health, Minutes, 1805-19, pp. 109-11, MS., photostat courtesy of Municipal Archives & Records Center, New York; letter from James Katsaros, Administrator, Municipal Archives and Records Center, 6 August 1957.

NATURAL HISTORY OF THE PASSIONS.

By Dr Carlton

Mihi crede, qui nihil agere videntur, Majora agunt; humana divinaq; simul tractant. Seneca Epist. 8.

In the SAVOY,
Printed by T. N. for James Magnes in Russell-Street, near the Piazza in Convent-Garden, 1674.

FIG. 1

book was listed among his works during his lifetime by Wood (1695) and continued to be so listed for two hundred years, for instance by Munk (1878). Dr. L. W. Hansen, Keeper of Printed Books, Bodleian Library, Oxford, kindly wrote to us that the Bodleian Library copy was received "presumably not very long after publication, and certainly in the author's lifetime. It was entered in the interleaved copy of our 1674 catalogue directly under Charleton's name, and so appears in subsequent catalogues of the Library." Charleton himself confirmed that he was the author in his *Enquiries into Human Nature, in VI. Anatomic Praelections* (London, 1680, p. 405), where he referred the "unsatisfied" reader who wanted further details about the interrelation of "the *Body* . . . the *Sensitive* or Vital *Soul* . . . and the *Spirit*" to "a little Treatise intitled a *Natural History of the Passions,* publish'd about three Years past, where the Author professedly handles it." And in the list of Charleton's works at the end of his *Three Anatomic Lectures* (London, 1683), under the heading *"Gualteri Charletoni scripta jam in Lucem emissa,"* item 17 is "A Natural History of the *Passions: Lond.* 1674. *Oct*[avo]."

Despite this unequivocal evidence, the British Museum's *General Catalogue of Printed Books* (vol. 36, 1943), mistakenly describes the book as "Based on *'De l'usage des passions'* by J. F. Senault"; Halkett and Laing (1928) state that it is "from the French of Jean F. Senault"; and Sir Humphry Rolleston (1940) in his brief bibliography of Charleton's writings—the only one yet attempted—called it simply "A translation from Senault," although he referred to Manning's (1926) checklist in which it is credited to Charleton without any reference to Senault. Wing (S 2501) presumably followed these authorities when he listed the work under Senault without any cross reference to Charleton. There is a cross reference to Senault from the entry by title.

Jean-François Senault (1601-1672), a French divine, published a book entitled *De l'usage des passions* at Paris in 1641. In 1649 a translation into English by Henry Carey, Earl of Monmouth, appeared in London as *The Use of Passions* (Fig. 2), and a second edition in 1671.

Comparison of Senault's *The Use of Passions* with Charleton's *Natural History of the Passions* shows no textual similarity between the two, with the possible exception of two sectional titles: thus the first part of Senault's book is headed "Of Passions in General" and the second part "Of Passions in Particular"; Section IV of Charleton's book is headed *"Of the* Passions *of the Mind in* general" and Section V *"Of the Passions in* particular." While Senault's book is an octavo with 510 pages of text, not counting a lengthy "Authors Dedication of his *Work,* To our Saviour Jesus Christ" nor "The Authors Preface," Charleton's work is an octavo of only 188 pages of text apart from the "Epistle Prefatory, *To a Person of Honor,* Friend to the Author," and contains only about a third of the number of words.

Senault's is essentially a moral or ethical treatise based on classical authors and written for the improvement of mankind: "Teach me the dex-

THE USE OF PASSIONS.

VVRITTEN IN
FRENCH
BY
J. F. Senault.

And put into
ENGLISH
BY
HENRY
Earl of MONMOUTH.
An. Dom. 1649.

London, Printed for *F. L.* and *Humphrey Moseley*,
at the Prince's Arms in St. *Pauls* Church-yard. 1649.

FIG. 2

terity we are to use, for conquering those *Passions* that *flatter* us with their *Promises;* those which *corrupt* us with their *subtilties;* those which *daunt* us with their *threats;* and those which *enhance* us with their *allurements:* That being *illuminated* by *thy Light,* and assisted by *thy Grace,* I may by *one* and the *same Work,* inspire mens souls with the Love of *Vertue,* and *Hatred,* of *Sin.*" Charleton's book on the other hand was written "because my accumulated Misfortunes had . . . reduced me to a necessity of consulting that part of Philosophy, about the most effectual *Remedies* against *Discontent.*" Like "the diseases of the *Body,*" he says, "so likewise those of the *Mind* may be more easily cured, when their nature and causes are understood," and "That all the Good and Evil of this life depends upon the various *Passions* incident to the Mind of man; I need no other document than my own dearly bought Experience." Wood from personal knowledge confirmed that Charleton was "a learned and an unhappy man, aged and grave" (Wood, Bliss ed., 1820), and this is confirmed also by the many references to melancholy in his own commonplace book (British Museum, Sloane MS.53).

Charleton set out to delineate philosophically and physiologically the interaction of body and mind (and soul), in other words to write a treatise on what is now called psychosomatic medicine, "to inquire . . . into the *nature, causes, motions* [effects] *&c.* of the *Passions*" and the "*Remedies* against their *Excesses.*" On the psychological side he dealt with such "modern" topics as instincts, conditioned reflexes, the pleasure-pain principle, theories of laughter and obsessional doubting, as well as melancholia, stupor, madness, and so on.

At the end of the book Charleton recommended "the *Ethicks* of *Epicurus*" as "(after Holy writ) the best *Dispensatory* I have hitherto read, of Natural *Medicines* for all distempers incident to the mind of Man." The second edition of his *Epicurus's Morals, Collected Partly out of his own Greek Text, in Diogenes Laertius, And Partly out of the Rhapsodies of Marcus Antoninus, Plutarch, Cicero, & Seneca. And faithfully Englished* (London, 1670) had been published four years previously. Incidentally, Wing served Charleton badly regarding this book also, for he does not mention his name in connection with it (Wing E 3156) or with the first edition of 1656 (Wing E 3155), although apart from the work of editing and translating, Charleton had written a lengthy signed introductory "Apologie for Epicurus."

As befits a physician, Charleton referred to "our Master *Hippocrates.*" The authors he quoted most frequently were "those three excellent Men, *Gassendus, Des Cartes,* and our *Mr. Hobbes,*" as well as Sir Kenelm Digby in "his Book of the *Rational Soul*" (*Two Treatises. In the one of which, the Nature of Bodies; in the other, the Nature of Mans Soule; is looked into.* First edition, Paris, 1644; London editions, 1645, 1658, 1665, 1669), and "the now flourishing" Dr. Thomas Willis in "his late Book *de Anima Brutorum*" (Oxford and also London, 1672) and "*de Anatomia Cerebri*"

(London, 1664). The relevant works of all these authors were published after the appearance of Senault's book in 1641.

We therefore set out to trace how and when Charleton's book came to be confused with the earlier work of Senault. It appears that the first to attribute *Natural History of the Passions* to Senault was Sir Norman Moore in his biography of Charleton in *The Dictionary of National Biography* (London, 1887). Later authorities seem merely to have copied his statement that the book was "A translation from the French of Senault."

How did Sir Norman come to make this error? His son kindly informed us that "he made a point of reading every work written by everyone whose biography he wrote . . . when he wished to look at a book that he had not himself he went to the British Museum" (Sir Alan Moore, personal communication)—and this is what he must have done in this instance, for his son tells us that he almost certainly did not possess a copy. On this information we retraced Sir Norman's steps and like him consulted the British Museum copy of Charleton's book and in it found that an unidentified late eighteenth century hand—probably a previous owner—had written on the title page "From the French of M. Senault." Here then was the explanation for the mistaken identification which had misled Sir Norman. Had he quoted his authority in the *D.N.B.*—and Sir Alan suggests that "for lack of space the Editor of the *D.N.B.* may have cut out a reference to the note in the British Museum copy"—the error might have been detected sooner and Walter Charleton would not have been deprived for seventy years by so many cataloguers and bibliographers of the authorship of one of his many books.

RICHARD A. HUNTER
EMILY CUTTLER

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Willan's *Cutaneous Diseases*

Dr. Beswick is to be congratulated on his interesting discovery that there are two, hitherto undifferentiated, versions of the text of Willan's *Cutaneous diseases,* and his analysis of the texts is most valuable (*J. Hist. Med.*, 1957, *12*, 349-65). When he attempts to resolve the bibliographical difficulties presented by its publication, his handling is less sure. Actually the mystery is not so great as Dr. Beswick assumes it is.

Serial publication in Willan's day, far from being a special measure made necessary by Willan's status or the uncertainty of success of his book, was a commonplace. Parts were sold as published and the title-page and index of the work (or volume, if more than one) included in the last part. It often happened that earlier parts of a work had to be reprinted in order to supply the demand for them by new subscribers. What is notable about Willan's text is that he took the opportunity of this reprinting to revise it, so that some subscribers, when they came to bind the volume, would have had a complete set of original issues, some a set made up of original and revised issues, while those who waited to buy until the volume was completed would be supplied from stock with parts of which only the last would be an unrevised issue.

From copies of Willan's text and other information available in the Wellcome Library, it is possible to trace the course of publication fairly clearly. Order 1 was first published in 1798 (for review with bibliographical details see *London Medical Review*, 1799, *1*, 30); Order 2 in 1801 (*ibid.*, 1801, *7*, 97); Order 3, Part 1 in 1805 (*Medico-chirurgical Review*, 1806, *12*, 23-4); and Order 3, Part 2 with Order 4 in 1808 (*Edinburgh Medical and Surgical Journal*, 1809, *5*, 73, and *Medical and Physical Journal*, 1809, *20*, 273, 371). Contemporary medical literature fails to provide any evidence that a single revised volume of the whole work was issued in 1808. The reviews and advertisements in medical journals for 1808 and 1809 are applicable only to a separately published fourth fasciculus comprising Order 3, Part 2 and Order 4. The Wellcome Library possesses a copy of this fasciculus still in its original paper wrappers, bearing the date 1808 and containing the title-page and table of contents for Volume I of *On cutaneous diseases*. The inclusion of these is mentioned by reviewers, but no reference is made by them to a complete volume, either already issued or planned for the future. The implication is clear that Volume I is completed by the fourth fasciculus and the reader is expected to bind his own copy, as was usual at that time with works published serially. If there was no separate publication of a volume with a revised text, then there must have been revised editions of the separate fasciculi. Proof of this fact can also be found in the Wellcome Library where there is a second edition of the third fasciculus (Order 3, Part 1) first published in 1805. Its front cover bears the words "The second edition" and the date 1807, and so explains the curious entry in Watt's *Bibliotheca Britannica* which reads "Ord. 3 Rashes, 1st Part, containing the varieties of Rubeola and Scarlatina, etc. Lond. 1805-7, 2 vols." The dates and the phrase '2 vols' must refer to the two editions of the third fasciculus.

Apart from the copies of the third and fourth fasciculi already mentioned, there are in the Wellcome Library two bound volumes of *On cutaneous diseases*, 1808. When these were tested by the appendices to Dr. Beswick's article, it was found that one version (Copy A) was clearly made up of earlier editions of the fasciculi; in the second (Copy B) the text of

the first three fasciculi is altered and compressed in the manner described by Dr. Beswick in the Cambridge University copy. The fourth fasciculus, however, is the same in both bound volumes and identical with the unbound version of 1808. When the unbound "second edition" of the third fasciculus was compared with the bound volumes, it was found to match exactly the relevant section in Copy B.

The fact that Watt and Allibone, whose admirable note on Willan *(Critical dictionary of English literature,* vol. 3, 1891) correctly dates the original issues, both refer obliquely to the second edition of the third fasciculus but not to subsequent editions of any other, is rather mysterious and may only be accounted for by the fact that the revised text of the first two fasciculi was issued with the same wrappers as were used for the original parts. This would indicate a need for caution in judging the issue of any part in wrappers and it seems that the issue can only be identified by a collation of the text.

The influence of Joseph Johnson, the publisher, on the course of this publication is of some interest. A biographical sketch of Johnson by the Noncomformist physician John Aikin can be found in Nichols' *Literary anecdotes of the 18th century* (1812, vol. 3, 461-4) from which it appears that he was, like Willan, a member of a dissenting sect and was also something of a radical in politics. The fact that he was imprisoned for nine months in 1797 for publishing the political works of Gilbert Wakefield may well have contributed to the delay in publication of the first part of Willan's work. He was a publisher of the highest integrity, but, in the words of Aikin, "he usually consulted cheapness rather than appearance in his own publications," and this pursuit of economy may have led him to issue amended versions of the first two parts in their original wrappers. If this is so, then only contemporary records can elucidate the actual date of their publication.

It will be seen that this evidence invalidates many of Dr. Beswick's arguments and authenticates the statements of Haldin-Davis and Pusey. There were not two editions of the volume, but only of the first three parts; and volumes found today may be made up of original and revised text in differing proportions. The parts were issued with a cover-title *The description and treatment of cutaneous diseases,* which was superseded by the new title *On cutaneous diseases* which appeared on the title-page for the volume published in 1808. It would be normal practice for librarians to continue to catalogue this work from the title-page, i.e. with the date 1808, but a note should indicate which version of the text is contained in the volume and should also inform the reader that the volume was published in parts between 1798 and 1808 with a cover-title *The description and treatment of cutaneous diseases.*

F. M. SUTHERLAND

Activity at Wisconsin

It is pleasant to announce that for the second semester of the academic year 1957-58 Prof. Walter Artelt will be visiting professor at the University of Wisconsin Medical School.

Professor Artelt, whose distinguished work in the history of medicine is internationally known, is Director of the Institute for the History of Medicine at the University of Frankfurt, Frankfurt am Main. A number of American scholars have been privileged to know him at international congresses. His valuable tools, *Einführung in die Medizinhistorik* (Stuttgart, 1949) and *Index zur Geschichte der Medizin, Naturwissenschaft und Technik,* volume I of which was published under his editorship by Urban & Schwarzenberg in 1953, are in constant use by historians. Fabrik E. Merck of Darmstadt issued in 1954 a series of articles by him carrying beautiful reproductions from the iconography of Saints Cosmas and Damian. In *Ciba-Zeitschrift,* 1956, *78,* he presented a history of the Medical Faculty at Berlin. The reprint of latest date to reach this department is from the *Rheinischer Merkur* for 7 June 1957 and concerns the catacomb fresco of the Via Dino Compagni in Rome, literature on which has been cited earlier in the Journal. Professor Artelt interprets the scene not as an anatomical demonstration or a clinical lesson but as a representation of the vision in *Ezekiel* 37 which became to many early Christians a prophecy of resurrection on Judgment Day. A skillful adaptation of the vision, from an article by Max H. Fisch, is quoted on page [vi] of Harvey Cushing, *A bio-bibliography of Andreas Vesalius.*

It is doubly pleasant to announce that Professor Artelt's wife, Edith Heischkel Artelt, who is a renowned historian in her own right and holds a chair at the University of Mainz, will come to this country later in the spring. Mrs. Artelt has worked particularly in medical historiography.

Historical studies in the State of Wisconsin will be promoted also by the contemplated Medical Museum at Prairie du Chien which will depict the progress made by successive generations of Wisconsin physicians. This project, which is sponsored by the State Medical Society, calls for the restoration or construction of four units, of which the backdrop will be Fort Crawford's Military Hospital, which was erected in the late 1820's. It was at this hospital that William Beaumont completed his basic studies in the physiology of digestion. Dr. William D. Stovall of Madison is general chairman of the campaign to raise $344,960 for the project, which it is hoped will be open to the public in two years.

Appointment at Kansas

Dr. Leslie R. C. Agnew, a Scotsman who has his M.D. from the University of Glasgow and an M.A. in the history of science from Harvard University, has been appointed Associate Professor of the History of Medicine and Chairman of the department at the University of Kansas. He suc-

ceeds Dr. Ralph H. Major, who became Emeritus Professor on 1 July 1955. *The Lancet* for 31 August 1957 (pp. 423-4) published an article by Dr. Agnew entitled "Medical history: profession or pastime?" The Journal extends its good wishes on this appointment which comes, in view of the new library building at the University of Kansas School of Medicine, at a particularly propitious time.

Visiting Lectureship for Prof. Max H. Fisch

Max H. Fisch, Professor of Philosophy at the University of Illinois and Chairman of the American Philosophical Association, will be visiting professor at Keio University in Tokyo, Japan, during its centennial year beginning in April 1958, by an appointment under the Fulbright Act.

During February and March 1958, Professor Fisch will lecture on American social philosophy at the following universities in India: Aligarh, Allahabad, Andhra (at Waltair), Baroda, Bombay, Calcutta, Delhi, Gujerat (at Ahmedabad), Lucknow, Patna, and Rajputana (at Jodhpur and Jaipur). His lecture tour in India has been arranged by the Inter-University Board of India in co-operation with the United States Educational Foundation in India.

Professor Fisch will attend the Pakistan Philosophical Congress at Hyderabad (Sind) 15-18 March, where he will read a paper and participate in two symposia. His attendance at this Congress has been made possible by the U. S. Department of State, at the request of the permanent president of the Pakistan Philosophical Congress, Professor M. M. Sharif of Lahore.

Until April, when Professor Fisch will be settled in Tokyo, Professor Charner M. Perry of the University of Chicago will serve as Acting Chairman of the American Philosophical Association. Professor Fisch's Tokyo address will be c/o International Department, Keio University, Mita, Minato-ku, Tokyo, Japan.

New Curator of the Streeter Collection of Weights and Measures at Yale

Dr. Bruno Kisch of New York has recently been appointed Curator of the Edward Clark Streeter Collection of Weights and Measures at the Yale University School of Medicine. For the past two years he has been cataloguing the individual items with the expectation that a printed catalogue will be published so that the many treasures in the Collection may become more widely known.

Dr. Kisch holds the title of Emeritus Full Professor (Ordinarius) of Physiology, Biochemistry, and Experimental Medicine at the University of Cologne, and at present is Professor of Philosophy and History of Science at Yeshiva University as well as its Medical Director.

Marcello Malpighi, Embryologist

In the *Journal of the American Medical Association* for 14 September 1957 (p. 158), Karl F. Meyer calls attention to Claudio Fermi's statement in his *La rabbia* (Siena, 1950, vol. 1, p. 42), that the mother of Marcello Malpighi is supposed to have died of rabies as a result of being bitten by her daughter (*La madre di Malpighi sarebbe morta di rabbia in seguito alla morsicatura infertale da sua figlia.*). Fermi does not document his statement, and Prof. Howard B. Adelmann knows of no other author who has even suggested this. Malpighi himself merely tells us in his autobiography that in 1649 he lost his father, mother, and a grandmother within a few days (*Anno 1649. orbatus patre, matre, & avia* [*paucis diebus fato sublatis*]). Can anyone supply the source of Fermi's statement?

Professor Adelmann is in the late stages of his forthcoming work on Marcello Malpighi and the evolution of embryological thought. The work, which will require two or three folio volumes, will consist of (1) translations into English of Malpighi's embryological treatises, (2) an exhaustive analysis of these works, and (3) extensive commentaries tracing, by means of translations of critical passages, the history of thought on the large number of topics touched upon by Malpighi; these histories often cover the period from the Greeks to the beginning of the modern epoch in the nineteenth century. There will also be a brief history of embryology up to Malpighi's time, a life of Malpighi based upon original documents, and a history of the Studium at Bologna in the seventeenth century.

It is expected that the first draft of the manuscript, upon which Professor Adelmann has been engaged for thirteen years, will be completed by the spring of 1958 and that it can be readied for the press by a year hence.

American Association of the History of Medicine

The Thirty-First Annual Meeting of the American Association of the History of Medicine will be held in New York City, 22-24 May 1958. The Association's hosts will be The New York Academy of Medicine and The Rockefeller Institute for Medical Research.

Members who may wish to present papers are requested to communicate with the Chairman of the Program Committee, Dr. Iago Galdston, The New York Academy of Medicine, 2 East 103 Street, New York 29, N. Y. Those submitting papers should send, preferably, the full text of their communication or a full abstract not less than five hundred words in length. No paper submitted by title only will be considered.

New Hall of Health at the Smithsonian Institution

A new Hall of Health in the U. S. National Museum was opened to the public by the Smithsonian Institution on 3 November 1957. The Hall's primary function is to present basic information on human anatomy and physiology to the layman. To this end there are exhibits on embryology,

birth and growth, and on the skeletal, muscular, cardiovascular, digestive, respiratory, endocrine, and nervous systems. The central feature of the Hall is a transparent woman which demonstrates the location and explains the function of the major organs of the body. However, in keeping with its theme "Through the ages, man's knowledge of his body," the Hall of Health also includes five historical panels showing by means of models and illustrations reproduced from medieval manuscripts and the works of Vesalius, Harvey, Leonardo da Vinci, and others, the growth of anatomical and physiological knowledge. Thus the visitor may learn something of the long history of the development of human biological science.

Of particular interest to medical historians is the fact that the new Hall does not replace but rather supplements the existing Smithsonian exhibits relating to the history of medical, dental, and pharmaceutical instruments and technology. Neighboring galleries will continue to exhibit the X-ray tubes, stethoscopes, sphygmomanometers, electrocardiographs, surgical instruments, apothecaries' balances, drug jars, and other historic specimens which are now there. In the future, moreover, these sections will in turn be renovated, in preparation for the Halls of Medical History and Pharmaceutical History which are to be included in the new Museum of History and Technology that is to be built in Washington within the next few years.

Planning for these new Halls is now going forward actively. In order that they may present the history of medicine to the public in as interesting, educational, and accurate a manner as possible and in a manner that will do justice to the medical profession, the National Museum's collections of representative medical, surgical, dental, and pharmaceutical instruments, devices, and equipment should be augmented. Physicians and others who have appropriate material which they might wish to donate to the National Museum are urged to write describing their specimens to either Mr. George B. Griffenhagen (pharmaceutical history) or Dr. John B. Blake (medical and dental history), Division of Medical Sciences, Smithsonian Institution, Washington 25, D. C.

JOHN B. BLAKE

Dr. Carlos J. Finlay

On 22-23 September 1955 (Journal, 1955, *10*, 433-4) the Jefferson Medical College, Philadelphia, commemorated the centennial of the graduation of Dr. Carlos J. Finlay. The commemorative ceremonies have now been fully reported, under the editorship of Sr. César Rodríguez Expósito, Historian on Sanitation, in *Booklet on sanitation history*, 1956, *10*, which is published by the Ministry of Health and Social Assistance, Havana. The issue carries a preface by Dr. Carlos Salas Humara, Minister of Sanitation and Social Assistance in Cuba; it includes, amid a wealth of illustrations, the proclamation of the governor of Pennsylvania on the occasion, the resolution passed in the Senate of Pennsylvania, an account of the life and

work of Finlay, the program of the ceremonies, a history of yellow fever to 1905 by Dr. Pedro Nogueira, a discussion of the doctrine of Finlay by Dr. Horacio Abascal, and an account of the unveiling of a bust of Finlay at the College and the conferring of honorary degrees by the College and of the Order of Merit Carlos J. Finlay by the Cuban Government.

In 1957 the Ministry of Social Assistance issued in mimeographed form the text of the remarks on Finlay by the Hon. Sol Bloom of New York in the House of Representatives, 28 October 1943. For this publication also, the Journal and historians are indebted to Sr. César Rodríguez Expósito.

Edward Mordake

This department of the Journal, having eliminated the word "Queries" from its name, is now of course receiving an occasional query. Eventually it may need to adopt the name "Notes and Querevents" which has been facetiously suggested by Dr. W. W. Francis. At the moment it would be glad for information, on behalf of Professor Paul G. Brewster of the Department of English, Tennessee Polytechnic Institute, concerning the story related from unidentified lay sources by George M. Gould and Walter L. Pyle in *Anomalies and curiosities of medicine* (New York, Julian Press, 1956, reprinted from the original edition of 1896), pp. 188-9. A young Englishman of noble birth, graceful face and figure, and fine attainments was cursed with a second face on the back of his head, a malignant female face which drove him to suicide in his twenty-third year. The young man is called Edward Mordake, his physicians are said to have been Manvers and Treadwell. If this is a genuine case in teratology, there should be authoritative sources for it. The assistance of readers is invited.

Historical Gleanings

In Samuel Noah Kramer's *From the tablets of Sumer: twenty-five firsts in man's recorded history* (Indian Hills, Colorado, Falcon's Wing Press, 1956) is included the "first pharmacopoeia," of the third millennium B.C., which was the subject of the Fielding H. Garrison Lecture for 1955 of the American Association of the History of Medicine. An abstract of the lecture is given in *Bulletin of the History of Medicine*, 1956, *30*, 41-2. The tablet is discussed by John L. Webb in *Bulletin of the Medical Library Association*, 1957, *45*, 1-4.

The *Prometheus Bound* of Aeschylus is replete with medical imagery which is scrutinized by Barbara Hughes Fowler in the *American Journal of Philology*, 1957, *78*, 173-84.

Robert Devreesse, formerly Vice-Prefect of the Vatican Library, published in 1954 (Paris, Klincksieck) an *Introduction à l'étude des manuscrits grecs*. In this, Chapter XVIII (pp. 259-72), as Jean Théodoridès points out in *Archives internationales d'histoire des sciences*, 1957, *10*, 41, is devoted to

medicine and provides a list of more than two hundred medical authors with their principal works. The list is based on H. Diels, *Die Handschriften der antiken Ärzte* (Berlin, 1905-6) but supplements that treatment in some respects.

Louis Cohn-Haft re-examines the problem of *The public physicians of ancient Greece* in Volume 42 (1956, x, 91 p.) of the *Smith College Studies in History*. Part I sets forth the problem, Part II considers the question whether these public physicians were an institution for social welfare or for the general welfare, Part III presents concluding remarks. The work is provided with detailed documentation, a bibliography, an appendix of principal inscriptions, and an index of sources—literary, epigraphical, papyrological, etc.

The excavations by Dr. Photios Petsas at Pella, birthplace of Alexander the Great, have unearthed an elaborate water system with pipes of terra cotta. Ancient Pella, which was razed by the Romans in 168 B.C., is near a modern village of the same name about twenty-five miles northeast of Salonika. Close at hand are springs which were known to the ancients. Important examples of mosaics have been found, including one covering eighty square feet which represents the youthful Dionysus riding joyfully upon a panther. A brief account of the campaign is given by A. C. Sedgwick in *The New York Times* for 16 September 1957. The work continues at what Dr. Petsas believes may be the royal palace.

On 9-11 May 1957 an International Symposium on Psychotropic Drugs was held in Milan with the support of the pharmaceutical companies Ciba, Farmitalia, Laboratori Glaxo, Union Clinique Belge, and Wyeth. Those who attended were reminded of historical background by an attractive 8-page brochure on the mandrake prepared for the occasion by Luigi Belloni and printed in both English and Italian.

Howard Stone of the Department of Modern Languages, College of William and Mary, is engaged in preparing an annotated edition of the surgical treatise of Abulcasis as it occurs in the French translation which was made in the thirteenth century by an unidentified hand. This translation, which shows many traits of Lorraine, survives in a manuscript of the same century at the Bibliothèque Nationale, fr. 1313. It omits or condenses many short passages while supplying the illustrations of instruments basically as they appear in the Latin translation by Gerardus Cremonensis which was published at Strassburg in 1532 with the *Rerum medicarum libri quatuor* of Theodorus Priscianus. Professor Stone's work is well advanced, and it is much to be hoped that proper arrangements can be made for publication.

The Library, Fifth Series, 1957, *12*, 11-22 carries a paper by Franklin B. Williams, Jr., of the Department of English of Georgetown University, on the index of dedications and commendatory verses on which he has been working for more than twenty years. The scope of the project is determined

by the *Short-title catalogue of English books, 1485-1640;* the index will enable instant location by STC number, in the original or prospective edition, of every book or part of a book dedicated to any individual, any city, any institution, or even any fanciful abstraction, and will cite by author or by first line of anonymous verses every commendatory poem, blurb, editorial preface, or other epistle not by the author of the book or by its translator. The process has involved personal examination of more than 30,000 items, including 3,000 editions or issues not yet in STC and some 650 unrecorded titles. Naturally, a large number of citations will be in the field of medicine. Each individual cited will be identified, and historians will be able to study far more carefully than before the relations between physicians and also the whole problem of patronage. Various titillating aspects will be immediately evident—succession in office, e.g., reflected in changes of dedication, as when Thomas Geminus dedicated successive editions of his "Compendiosa" to Henry VIII, Edward VI, and Elizabeth I, and the factotum epistle, in which blanks were left for completion in manuscript. These served very practical purposes, as when Walter Bayley presented his pamphlets to his friends as New Year's gifts with the name of each friend duly inscribed in the convenient blank. The importance of all these minor literary efforts was already apparent to some degree from Karl Schottenloher's *Die Widmungsvorrede im Buch des 16. Jahrhunderts,* Münster Westfalen, 1953 (*Reformationsgeschichtliche Studien und Texte,* Heft 76/77), which likewise cited physicians. Historians will hope for speedy completion and publication of Professor Williams's index.

In the *Bulletin hispanique,* 1956, 57, [182]-200 appears a study by Vincenzo Busacchi entitled "Gli studenti spagnoli di medicina e di arti in Bologna dal 1504 al 1575."

V. L. Saulnier's article "Médecins de Montpellier au temps de Rabelais" in *Bibliothèque d'humanisme et Renaissance,* 1957, *19,* 425-79 forms a supplement to Marcel Gouron's *Matricule de l'Université de Médecine de Montpellier (1503-1599),* which appeared as No. 25 in *Travaux d'humanisme et Renaissance* (Genève, Droz, 1957). Further information is supplied in an alphabetical list on some of the physicians cited by Gouron, and an index is provided of students omitted from the matriculation lists but known from some other source.

Francesco Guicciardini relates in Book XIII of *La historia d'Italia* (1st ed. 1561-4) the attempt of Cardinal Alfonso Petrucci in 1517 to poison Pope Leo X with the help of the surgeon and syphilologist Giovan Battista da Vercelli. The result was to be obtained by tampering with a fistula with which the Pope had long been afflicted. But the plot was discovered, and after due trial the cardinal was strangled in Castel Sant'Angelo, the surgeon publicly hanged and quartered though he never confessed even under torture and in fact protested his innocence to the end. The matter is briefly mentioned by Gaetano Marini in *Degli archiatri pontificj* (Roma, 1784),

1, 319 and is more fully discussed, with careful documentation, by Pietro Capparoni in *Boll. dell'Istituto storico italiano dell'arte sanitaria*, 1921-22, 3-56, 5 pl. It has now been re-examined by Fabrizio Winspeare, a physician and historian of Florence, in *La congiura dei cardinali contro Leone X* (Firenze, Leo S. Olschki, 1957), which forms volume V of the *Biblioteca dell'archivio storico italiano.*

L. R. Lind of the Department of Latin and Greek, University of Kansas, has completed a translation of the *Isagogae breves* of Berengario da Carpi. It is based on the Venice edition of 1535, but the 1522 and 1523 Bologna editions were collated against this and all significant variations were taken into account. The work has been accepted for publication by the University of Chicago Press; it will include an introduction on the life and work of Berengario and a set of historical notes by Lind identifying Berengario's quotations. A set of anatomical notes by Lind's colleague Paul G. Roofe of the Department of Anatomy will identify in their modern version Berengario's anatomical terms. Readers will doubtless find it of interest to compare with this translation that of Master Henry Jackson, Chirurgeon, published in London in 1660 and 1664, to which Sanford V. Larkey and L. Tum Suden called attention in *Isis*, 1934, *21*, 57-70 and which is duly listed in Vittorio Putti's bio-bibliography of Berengario (Bologna, 1937, pp. 156-60).

An illustrated article by Dr. Jadwiga Schwann and Dr. Stanislaw Schwann on Polish dermatologists of the Renaissance is presented in the *Wissenschaftliche Zeitschrift der Ernst Moritz Arndt-Universität*, Greifswald, Mathematisch-naturwissenschaftliche Reihe Nr.1/2, 1956/7, *6*, 103-15. Contributions by Wojciech Oczko (1537-99), Bartlomiej Hierowski (Hierovius, c.1565-1612), Jan Jonston (1603-75), and others are discussed.

In the *Annual Report* of The John Carter Brown Library, 1955-56, pp. 42-9, Lawrence C. Wroth signalizes the notable contribution made by Dr. Nicolás Monardes of Seville to pharmacology when in 1565 in his *Dos libros* he coördinated the knowledge which he had long accumulated of New World curative agents. A *Segunda Parte* in 1571 equaled in size the original contribution, and the new knowledge was then carried over the Continent by the Latin translations of Charles de l'Escluse, beginning in 1574, and in 1577 by John Frampton's English translation which bore the inspired title *Joyfull Newes out of the newe founde worlde.* Dr. Wroth points out that *Joyfull Newes*, ostensibly and primarily a medical treatise, was also a tremendous stimulation to English curiosity about the New World and so a political instrument which played its part in later colonization. The John Carter Brown Library has recently acquired the 1580 edition, to which Frampton, urged by the celebrated London physician Dr. Hector Nunes, a Portuguese Jew, added parts which he had omitted earlier on the use of iron in medicine, on snow or ice as a refrigeratory agent, and on the value of the bezoar stone in the treatment of poisoning.

Tradition has long held that beneath a slab of marble to the left of the High Altar in San Marco on which are carved a doge's hat and a heart lay, in fact, the heart of a doge. In the spring of 1956, during restorations to the pavement, this slab was raised. Beneath it was found a wooden box containing a marble urn in which was the calcified heart of Francesco Erizzo, doge of Venice from 10 April 1631 until his death 3 January 1646. His body lies in the mausoleum erected in 1633 in the church of San Martino. It was this doge who on 28 November 1631 proclaimed the city free of the pestilence which had taken a toll of almost 47,000 victims. He initiated the construction of the church of Santa Maria della Salute to fulfil a vow made by the citizens for liberation from the pestilence. Readers will recall that in the Sacristy of this renowned and lovely church is the painting done by Titian in 1512 of St. Mark between Saints Cosmas and Damian on the left and Saints Roch and Sebastian on the right.

The William Harvey issue of the Journal, April, 1957, was reviewed by F. N. L. Poynter in the *British Medical Journal,* 1 June 1957, 1297-9.

In *Notes and Queries,* New Series, 1957, *4,* 108-12, A. G. E. Jones describes the great plague which afflicted Yarmouth from November of 1664 through September of 1666. It arrived from Amsterdam, with which Yarmouth had at that time close relations of trade. The parish register of burials records in all 1,144 deaths.

In *The Slavonic and East European Review,* 1957, *35,* 551-62 Nicholas Hans, Reader in Comparative Education at the University of London, makes a study of Russian students at Leyden in the eighteenth century. He lists chronologically by year of matriculation, with name, age, faculty wherever known, place of birth, and a few notes, 143 students, of whom 52 studied certainly in the faculty of medicine.

Something of the earlier history of the Board of Health of New York City is set down near the beginning of this issue of "Notes and Events." For a later period Dr. Leona Baumgartner, Commissioner of Health, cites in a letter to the Editor of the Journal an article by T. M. Rivers in the *Journal of Experimental Medicine,* 1931, *54.* On page 453 of this article the statement is repeated from the Yearly Report of the Board of Health for 1874-75 that vaccinating was begun with virus of the same stock as that which Dr. Loines of the Eastern Dispensary in New York had supplied for about five years and which he had used and sold for about twenty years previously. This virus was originally obtained by Dr. Loines from England and was very probably descended from the stock furnished by Jenner. Use of the same strain was continued in the twentieth century, the only alteration being that the virus was "humanized" at various times, usually one to three times yearly.

On 7 February 1956, at the age of 82, the Italian medical, literary, and economic historian Dr. Luigi Messedaglia died at his villa near Verona. An

obituary by Ettore Bonora was published in the *Giornale storico della letteratura italiana,* 1956, 484-6. On 12 May 1957, a commemorative address, written by Prof. Francesco Pellegrini, was read before the Accademia di Agricoltura, Scienze e Lettere of Verona by Mons. Giuseppe Turrini, librarian of the Biblioteca Capitolare in that city. Professor Pellegrini's own contributions to medical history, particularly on the subjects of military medicine and Fracastoro, are familiar to readers of the Journal. An article of his entitled "Conformisti o non conformisti" and bearing on the disputed role of rats in the transmission of plague appeared in the *Pensiero medico* of Milan for 20 April 1957.

The *Bible Society Record* for June, 1957, contains on page 78 a Memorial Minute on Dr. Edward H. Hume (Journal, 1957, *12,* 267-8) adopted by the Board of Managers of the American Bible Society on 7 March 1957. Dr. Hume had been for twelve years a member of this Board.

At Macdonald College Summer School from 2-20 July 1957, a Science Workshop for high school teachers was given by M. J. Dunbar, Associate Professor of Zoology at McGill University, G. H. N. Towers of the Botany Department, and R. F. Rivard, Vice-Principal of Montreal West High School and teacher of physics and chemistry. The Dupont Company supported the project financially and provided scholarships for the teachers who enrolled. The purpose of the course was "to develop new ideas in high school science teaching, to open new perspectives in the nature of the natural sciences by emphasis both on their history and on their immediate future . . . , and to show that by the exercise of imagination and by keeping in touch with science at its frontiers, the textbooks . . . might be leaned upon less heavily." Five mornings were devoted to the history of science, and the importance of a knowledge of it was emphasized, "both as the best means of understanding science as such and as a way to develop a frame of reference within which to teach it." A goodly amount of enthusiasm and even of excitement was generated. It is hoped that the workshop can be offered every summer.

The eightieth birthday of Dr. Bela Schick on 16 July 1957 was warmly noticed in *The New York Times* for 12 June 1957 and *Life Magazine* for 28 October 1957.

A year ago it was noted in these pages (1957, *12,* 85) that the Trudeau Sanatorium at Saranac Lake, New York, was available for purchase. *The New York Times* for 1 October 1957 reports that the property was acquired by the American Management Association and converted into an Academy of Advanced Management which becomes the world's largest resident training facility for business executives. A far cry indeed from the rest cure.

Readers who would enjoy at one and the same time the wisdom of a great neurologist and the conversation of a distinguished poet should dip into Sir Russell Brain's *Tea with Walter de la Mare,* which was published

in 1957 by Faber and Faber. From 1951 to 1956 Sir Russell was often a guest for tea at the poet's house. This book is the record of the topics discussed and the remarks made by his host.

In November of 1957 Gorgas Hospital in the Canal Zone, which was begun by the French in 1881, acquired by the United States Government in 1904, and renamed for Dr. William Crawford Gorgas in 1928, celebrated its diamond jubilee. Appropriate ceremonies were held at it and at its three subsidiary hospitals, and a commemorative 3-cent stamp was issued by the postal department of the Canal Zone. The events are reported in *The New York Times* for 24 November 1957.

D. J. Davis Lecture

Dr. Ilza Veith of Chicago will deliver the annual D. J. Davis Memorial Lecture on Medical History at the Medical-Dental-Pharmacy Building of the University of Illinois College of Medicine on 16 April 1958. Her subject will be "Oriental Medicine and its Concepts of the Soul."

First Pan-American Congress of the History of Medicine

The First Pan-American Congress of the History of Medicine will be held at Rio de Janeiro 12-18 April under the Presidency of Prof. Dr. Ivolino de Vasconcellos. Dr. Ordival Cassiano Gomes is Secretary-General and Dr. Paulo R. Bandeira, Treasurer. It is hoped that a number from North as well as South America can attend. The 14th of April will be Pan-American Day when a Pan-American Academy of the History of Medicine will be organized to promote relations between medical historians of both continents.

The Congress will mark the 150th anniversary of the beginning of medical teaching in Brazil and one of the themes to be discussed will be The History of Medical Teaching. The other principal theme will be Pre-Columbian Medicine. Papers to be presented should not be more than 30 pages in length (double spaced typing) and should be sent to the Secretary before 12 March.

Dr. Francisco Guerra, who is Visiting Lecturer in the History of Medicine at the Yale University School of Medicine, is preparing a *Bibliografia Médica Brasileira período colonial 1808-1822* for this occasion.

Book Reviews

AUGUSTO BECCARIA, *I codici di medicina del periodo presalernitano (secoli IX, X, e XI)*. Roma, Edizioni di storia e letteratura, Via Lancellotti 18, 1956. 500 pp. 4500 lire.

Reviewed by DOROTHY M. SCHULLIAN.

TWENTY years ago in the solid and authoritative *Rivista storica italiana* (Serie V, 1937, 2, 17-52) Professor Beccaria sent up the trial balloon for this catalogue in the form of an article entitled "Il ritorno della scienza classica nel primo medioevo." Even then it was clear to him that medicine's part in the return was important, a thesis which is fully supported by his completed catalogue. The catalogue would be indispensable, as an Italian bookseller has put it, to every library and every student of the Middle Ages for its indexes alone; its value for the history of medicine is immense, and to historians who recall Henry E. Sigerist's initial efforts in the same direction the catalogue will be of special interest. To Sudhoff, to Wickersheimer, to Sigerist who could not carry to fruition his plans, to other predecessors in Europe and in America Beccaria gives the credit which is their due while revealing how essentially original and unremitting his own toil has been. They would all rejoice in his success and commend the many librarians who furthered his researches; they would likewise proclaim unworthy of their calling the few librarians, mercifully left nameless, who refused to cooperate.

In an introduction of almost eighty pages the compiler sets forth the purposes and principles which have guided him. Medicine because of its practical bent was less affected than other disciplines by the pervading influence of religion, and it can therefore illustrate better the survival of classical knowledge and its transformation into a new product as the peoples of Europe began to shake off the lethargy of centuries. The early Middle Ages were a period of transition and preparation; "unica fra le scienze, la medicina si mette in cammino," and the awakening at Salerno is presaged in Latin manuscripts of the ninth, tenth, and eleventh centuries whose content is here for the first time fully analyzed and evaluated. This is the great contribution of the volume, to marshal into orderly arrangement the manifold tracts, identified and anonymous, which occur repeatedly in the manuscripts and which are vital to an understanding of medicine's position in this period and of its later development. "Bits of legendary biography, strings of epistles addressed to great names of the past, proemiums and extracts, bits about plants and animals, recipes and dinamidia, rules for phlebotomy, dream books and moon books, charms, lists of lucky and unlucky days," as Lynn Thorndike has expressed it (*Speculum*, 1957, 32, 536-7), are here together with Hippocrates and Galen, Antonius Musa and Caelius

Aurelianus and Alexander Trallianus, to show us the condition of medicine before Salerno and Constantinus Africanus.

There were many pitfalls, palaeographical in particular, which have been skillfully avoided. The volume bears witness throughout to the good faith of its compiler, to a desire to judge fairly and not press the evidence too far. Dating in general is by century only; manuscripts which bridge the eighth and ninth and the eleventh and twelfth centuries are included; and wherever the choice of date diverges from a date previously assigned to a given manuscript, the contrary opinion is also set down. Every possible means of localizing a manuscript is considered: script, corrections, glosses, marginalia, inscriptions of ownership, binding. The testimony of early inventories is adopted only where identification is probable; the inventories are, as usual, heartbreaking both for their vagueness and for the confirmation they give that only a part of the production for the period under discussion is now extant to us. With that part, however, it is our duty to deal as fully as we can, and it is not probable that the conclusions which we draw from it would be drastically changed if we had the whole. Beccaria catalogues 158 manuscripts contained in 145 codices now in 12 countries of Europe; of the manuscripts 117 are truly medical and 41 are miscellanies which include some medical portions. They contain 1,098 texts. Two (his nos. 98 and 99) are palimpsests, one (no. 124) is a rotolo. One (no. 1!) was carried off from Salzburg during World War II and is now missing; two at Chartres (nos. 11 and 12) were destroyed by bombing during the same war; a third at Chartres (no. 10) was reduced to a few charred fragments, but it is comforting to add here that the Bibliothèque Municipale now has microfilm of this manuscript made with the financial assistance of Israel E. Drabkin from photostats in the possession of Loren C. MacKinney. On the fate of some which he examined before World War II and which were housed in zones that suffered war damage, Beccaria is uncertain. One codex (no. 110) was destroyed in 1904 in the disastrous fire at the University of Torino. I am informed by Philip Robinson that Phillipps MS. 386, Hippocrates, which Beccaria (no. 72) saw through the courtesy of William Robinson and which was once bound with the renowned Apicius now at the New York Academy of Medicine, is at present in the Bibliothèque Bodmer, Cologny-Genève, Switzerland.

Of the 145 codices, Beccaria has examined personally all but sixteen, of which one was the Torino casualty. For several, Bernard Bischoff rendered invaluable assistance. Some have been catalogued from photographs. For the one at Cesena the compiler had the supreme authority of Augusto Campana, for the one at Ivrea, an edition by Giacosa, yet it is sad that he did not himself see all those in Italian libraries. Some manuscripts falling within his limits may have escaped his notice entirely, but he is probably correct in believing that they cannot be many. For each of his codices, and each medical manuscript within it, he sets down carefully under three main groupings the following information: writing material, dimen-

sions, number of folios, present ordering of the gatherings, the number and type of hands at work, listing of any illustrations, all indications of provenance, description of the binding; content by author and/or title, incipit and explicit, title of the last chapter, internal divisions of the larger works; a chronological bibliography. To the testimony on the great Vatican Celsus, lat. 5951, may be added appropriately that of Domenico Cotugno, who examined it in the spring of 1765. There are numerous cross-references, and into two precious indexes at the end are gathered first the incipits in alphabetical order, with references to the codices 1-145 in which they occur and to the specific tract, by number, within those codices, and then authors and/or titles in alphabetical order. The measure of the labor and analysis which were required here is clear from the fact that for "Calendari dietetici" twelve different types are listed, each with its group of numerical references. When we are given so much it is ungrateful to ask for more, but whoever works with provenance will long for an index also in this important field.

One needs to know Italian to reap full benefit from the volume itself and from its indexes. It is simple enough to remember that Stoccarda stands for Stuttgart, Treviri for Trier, but Italianization of names persists for the authors, and the index under the letter H gives no clew to Eliodoro, Ermogene, Erofilo, and Ippocrate. The alphabetization is occasionally strange (Quinto Sereno under Quinto, Sesto Placito under Sesto), and Italian typesetters need to be reminded once more that the English language is not syllabicated on the principles of the Italian and that Charles is not abbreviated Ch. In a few cases the reader, referred to another entry, is in turn referred from that one to a third. But the accuracy throughout is so phenomenal that owners will wish to correct in their copies a few cases which might prove treacherous in the use of the indexes: p. 410, line 28, read 88 for 87; p. 414, line 11, 36 for 33; p. 433, line 20, 141 for 140; p. 453, line 13, 95 for 94; p. 476, line 24, 36 for 35. Other misprints here and in the body of the catalogue are not of the kind which will mislead.

Palaeographers, medical librarians, and historians of medicine will do well to ponder upon two facts which seem to emerge from these pages. In the first place, perhaps a dozen of these manuscripts are mere fragments which have been recovered from bindings. It has become increasingly evident of late, as cataloguers have penetrated the secrets of the less well-known libraries, that the era of great discoveries in Western manuscripts has in general passed and that for new texts we must look now to fragments of this sort and to the earlier writing on palimpsests, in short, to pieces only. We should still be grateful for the lost books of Livy, the entire encyclopaedia of Celsus, the full Soranus behind Caelius Aurelianus, but hope of finding them is dimmer than it was a century ago. In the second place, historians of medicine who would edit manuscripts of Beccaria's period or later must be linguistically and palaeographically trained; one glance at his bibliographies for the separate codices, or at the bibliography

on pp. 94-102 which lists in alphabetical order items dealing with more than one codex, reveals both how much of the work to date has been done by classicists and how much has appeared in nonmedical journals. In the acquisition of such material in the form of reprints or photocopies medical librarians would seem to have a very special responsibility.

To Professor Beccaria a cordial "Well done!" and to Msgr. Giuseppe De Luca our gratitude for his continued encouragement of scholarly publishing.

GEORGE SARTON. *Six Wings. Men of Science in the Renaissance.* Bloomington, Ind., Indiana University Press, 1957. x, 318 pp., illus. $6.75.

Reviewed by C. D. O'MALLEY, *Professor of History, Stanford University, California.*

THE work presently under consideration represents an expansion of the Patten Lectures delivered by Dr. Sarton at the University of Indiana in 1955. The title is a reference to the six wings of the seraphim (Isaiah 6:2), and as presented by Dr. Sarton the wings are, respectively, "The Frame of the Renaissance," "Mathematics and Astronomy," "Physics, Chemistry and Technology," "Natural History," "Anatomy and Medicine," and "Leonardo da Vinci." In the notes to this last wing the reader is informed that in 1916, in conjunction with an invitation to deliver a series of lectures on science in Leonardo's age, Dr. Sarton came to realize that his knowledge of the necessary background of ancient and medieval science was insufficient for the purpose. It was through his efforts to repair that weakness that he was led to the research which produced his monumental *Introduction to the History of Science.* Regrettably, that work never progressed as far as the time of Leonardo, so that in a sense he never achieved his goal, and it is somewhat disappointing to find here only fourteen pages devoted to the man who was the original impulse behind Dr. Sarton's major contribution to the history of science. On the other hand, by allowing somewhat flexible dates for the age of Leonardo, these lectures can be said to represent that series of lectures which he was invited to give approximately forty years earlier.

Both historians so-called and historians of science have from time to time been guilty of ignoring the efforts of each other and so in some degree of working in a kind of vacuum. While there has come to be less and less of this sort of scholarly provincialism, no one has made a greater effort than Dr. Sarton to produce a recognition of history as a great synthesis of which science is one of the important ingredients and to demonstrate the influence of science in that synthesis.

The history of science provides the axis, or, let us say, the armature of the history of mankind. The armature is essential but insufficient. A man needs a good skeleton to be sure, but that is not enough; he needs blood and flesh, and even that is as nothing, he needs feelings and thoughts; then only is he complete.

My work has always been inspired by that ideal. I am primarily a historian of science, but the history of science has never satisfied me in itself; I always needed and gave away much more.

All that Dr. Sarton has written is worthy of study and this book no less than its predecessors. Nor should one ever ignore his notes, always full of startling bits of information and Sartonian convictions. In a review of this brevity it is, of course, impossible to consider all the Renaissance men of science treated in the book or, indeed, even those in any one of the wings. Since, however, Dr. Sarton has been compelled to make a selection of those men whom he considers as most important and representative, it is natural that the reader will debate some of the selections and query the absence of others. Thus in the presentation of those men associated with Renaissance medicine, although it must be conceded that these men are certainly of importance, it is regrettable that space could not be found for Berengario da Carpi or for Niccolò Massa. However, within the limits proposed by the author one can have only praise for his presentation.

ALLARDYCE NICOLL. *The Elizabethans.* Cambridge University Press, 1957. 174 pp. $5.00.

Reviewed by F. N. L. POYNTER, *Librarian, The Wellcome Historical Medical Library, London, England.*

IN this delightful book we are introduced to the Elizabethans by one who probably knows them as well as anybody living. Professor Nicoll is an outstanding authority on the history of the drama and Director of the Shakespeare Institute at Stratford-on-Avon, and for the medical historian who is seeking to understand Elizabethan England no better guide could be found. This is the land in which the Pilgrim Fathers grew up, and its people are allowed to give an image of their times in their own words and in their own pictures. One can see it as a brilliant 'documentary' made with the greatest possible care and thought to communicate the sense of the past with which Professor Nicoll is so strongly endowed. The arrangement of the work reflects the national hierarchy which was one of the great political achievements of the Tudor dynasty and which is only now being broken up by the revolution which has given us the welfare state. There are sections on Queen Elizabeth herself, the Queen's subjects, government and justice, the Church, London, the countryside, the home, schools, universities and foreign travel, the army, the navy, and the arts, but those which will most interest readers of this journal are the sections on science and physic and the plague. The thoughtful commentaries which Professor Nicoll provides for each of his scenes is well exemplified by the introduction to that where we are shown science and physic and from which I quote a paragraph which should be borne in mind by all who attempt to study the medicine of the time:

Dimly, but only dimly, Elizabeth's England glimpsed the vision of a medical faculty, trained in the art of healing and powerful enough to prevent the malpractices of charlatans. So long as men thought it part of their education to become ministers of the sick at home, so long as wise women and wizards laid a spell on the popular mind, there was little possibility of a rational development of medical knowledge. And so long as both the unlettered ploughman and the learned scholar put faith in the hidden properties of things, exact scientific method was an impossibility.

It is rarely indeed that we can agree with all that a publisher claims for a book on its dust-wrapper, but for me the book is beyond criticism. One comes from it with a real grasp of Elizabethan life, gathered from what Elizabethans said and believed and made, what they looked like, how their lives were ordered, what they achieved, what they feared and enjoyed, what they did, wore, lived in—even what they ate off. All this is from their own words and what they made with their own hands—the surviving riches. This book does what it usually takes a library and a museum and some years' study to do; it does it with an immediate effect of life and directness, with no glass in front of it. Both illustrations and quotations are selected from a wide range of sources and none is hackneyed. The production is of the high standard which we have come to expect of the Cambridge University Press.

KENNETH D. KEELE. *Anatomies of Pain.* Springfield, Ill., Charles C Thomas, 1957. x, 206 pp. 14 figs., 13 plates. $5.50.

Reviewed by ROBERT B. LIVINGSTON, *Director of Basic Research, National Institute of Mental Health and National Institute of Neurological Diseases, National Institutes of Health, Bethesda, Maryland.*

THIS is a sweeping history of the growth of concepts relating to pain. The book deals primarily with the anatomical and physiological mechanisms believed to be implicated in the experiencing of this cardinal symptom—pain—but, more than this, it reflects man's continuing struggle to understand his own nervous system. This monograph is so successful that we naturally look forward to the historical treatment of other symptoms and signs. Dr. Keele writes engagingly and has selected material from many sources. There are thirteen plates and a number of figures: several rather choice ones are from the Wellcome Historical Medical Library.

In the beginning he briefly scans the interpretations of primitive man and non-Western cultures; it is interesting to recall how detached and at the same time perceptive are many of the great philosophical and religious systems of India and Asia, and how intimately they address themselves to problems we all share. Greek medicine and thought concerning pain lead by familiar paths to Leonardo, Descartes, and Willis. For every step of progress in men's thinking, there has been considerable slipping backwards betweentimes. In regard to the problems of pain, one has the impression that people of insight speak to us from any era and any culture with more

significance in their messages than most of what is bundled up in today's fifteen thousand journals.

Approximately half of the book is devoted to nineteenth and twentieth century advances relating to pain mechanisms: Bell, Bechterev, Gower, Brown-Séquard, Edinger, Ramón y Cajal, Müller, Gall, LeGallois, Flourens, Goltz, Flechsig, von Monakow, Foerster, von Frey, E. H. Weber, MacKenzie, Head, Lennander, Stopford, Leriche, Lewis, W. K. Livingston, Adrian, Tower, Moruzzi, Magoun, Bremer, Fulton, MacLean, Penfield, White, and Sweet, as well as many others. Finally, the author concludes with a chapter entitled "The Structure of Pain" which assembles his contemporary view. Dr. Keele's own research on chemical substances that are capable of initiating pain sensation has apparently led him to favor a view of fairly linear cause-and-effect relations between a given stimulus and a corresponding precept; perhaps he gives insufficient regard to the way in which precepts may be shaped by concurrent or antecedent neurophysiological events. He quite rightly points out the revolution in thinking that has come upon us recently through observations concerning chemical mechanisms involved in stimulation and transmission throughout all levels of neuronal integration; he speaks of this as "a new and fertile field, the biochemistry of pain." He also stresses the impact of work relating to the reticular formation and the centralization of the sensorium this provides. "The search for the Sensorium Commune has commenced again. This is a subject upon which we are fortunate to have at our disposal the views of the most able thinkers the world has known from Plato and Aristotle onwards. And it is in the conviction that a reinterpretation of the old ideas in terms of new knowledge constitutes a fruitful form of scientific thought that this attempt to tell the story of the anatomies of pain has been made." Altogether, Dr. Keele has made a valuable contribution to the history of disease.

THOMAS S. SZASZ. *Pain and Pleasure. A Study of Bodily Feelings.* New York, Basic Books, Inc., 1957. 301 pp. $5.50.

Reviewed by RICHARD A. HUNTER, *Psychiatric Registrar, National Hospital for Nervous Diseases, London, England.*

PAIN is the only sense modality which does not correspond to a physical state of matter as do the other senses. It is entirely subjective. In medical practice pain of some sort is the commonest presenting symptom and for purposes of diagnosis patients are grouped into those with organic disease and those without: "It is the doctor's daily labour to unravel the meaning of pain, whether it has a tangible seat or not," wrote Sir Samuel Wilks (*Lectures on diseases of the nervous system.* 2d edition, London, Churchill, 1883, p. 461). Often psychiatric patients complain primarily of physical dis-ease or pain.

In Part I of this book Dr. Szasz discusses "the mind-body problem in the light of the philosophy of science." In Part II he examines pain from the point of view of a psychoanalyst dissatisfied with psychoanalytic theory:

"It appears that pain has somehow been excluded from psychoanalytic theory. . . . The 'physical' is excluded from the domain of psychoanalysis altogether [53]. . . . It is my thesis that the very nature of the original concepts of id, ego, superego, and external reality preclude a psychoanalytic understanding of pain and certain other processes connected with the body" [55]. The reason for this is that psychoanalysis was built on a theory of instincts of love and hate, life and death, and the transference phenomena of the psychoanalytic session. In such a scheme there is no place for a person's relation to his own mind and his own body, the field in which pain arises and is experienced: intrapersonal events, if noted at all, are taken merely as echoes of interpersonal phenomena. Freud's only "interest in pain seems to have centered around his idea of the 'pleasure-pain principle' [74] . . . 'unpleasure [pain] corresponds to an *increase* in the quantity of excitation and pleasure to a *diminution*'" [75]. In this sense 'unpleasure' or pain is taken to mean an excess of excitation in the psychic apparatus of which physical pain is only one possible cause. In other words, the pleasure-pain principle does not refer to pain in its clinical meaning.

To fit pain into the interpersonal framework of psychoanalysis Dr. Szasz is forced to regard it "as an affect [82] . . . a signal informing the ego of a danger in relation to the body . . . a message requesting help from another person [88]. . . . In other words, if we look upon the body as an object of the ego, it may be possible for us to expand the concept of transference from human objects to the body [98]." That is, interpersonal libidinal drives are not only directed towards people, but also potentially centre on the patient's own body. In this way pain becomes "a potentially multivalued communication [101]."

In Part III, "the psychology of bodily feelings," the author attempts to show that all "bodily symptoms may represent transferences [145]." Not surprisingly this has led him into a number of clinical misjudgments. For instance, can a catatonic or hebephrenic state really be considered "much closer to virtual unconsciousness" [141] when in health such a pitch of exciting, vivid, and adventurous mentation is hardly possible without the help of hallucinogenic drugs, and when patients may subsequently show they have been "conscious" throughout by giving detailed accounts of daily events during years of apparent stupor? Referring to Daniel Paul Schreber, psychiatry's best-documented and most famous patient, he writes that "during his so-called recovery he was *certain* that he was a woman, indeed, God's wife [144]." This is not so: Schreber was convinced that he was changing into a woman in order to be fertilized miraculously by divine rays—a far cry from the sexual act being performed between God and himself as visualized by the author following Freud.

Part IV deals with "the psychology of pleasure." Pleasure—a mental state—as such rarely, if ever, presents clinically as an object of psychiatric or psychological investigation, and certainly cannot be taken as the obverse

of pain—a physical sensation. The author believes that just as pain "expresses an accusation directed toward a significant person or a demand for some sort of help from him [203]," pleasure is "an affect signaling a satisfying relationship between the ego and an important (human) object [207]." Here again there are clinical errors. One cannot accept that "the syndrome of mania . . . represents a kind of pathological exaggeration or a caricature . . . of certain other pleasurable experiences [207-8]." Observation of the mental state known as hypomania may possibly mislead a superficial observer: true mania, which is really indistinguishable from catatonic excitement, is terrifying to all concerned and used to carry a not inconsiderable mortality rate from exhaustion.

Part V, headed "retrospects and conclusions," contains *inter alia* a very debatable interpretation of Goethe's Faust, which epitomises how unsatisfactory and indeed impossible it is to interpret life as being played out on the pleasure-pain axis.

In summary, this book is a brave attempt to point out and mend a major deficiency in psychoanalytic theory. That the author has not succeeded in this interesting book is the fault of the fundamental tenets of psychoanalysis—the libido theory and the pleasure-pain principle—from which he is unable to free himself.

JOHN B. BLAKE. *Benjamin Waterhouse and the Introduction of Vaccination. A Reappraisal.* (Yale University Department of the History of Medicine, Monograph Series Number 33.) Philadelphia, University of Pennsylvania Press, 1957. 95 pp. $4.00.

Reviewed by HENRY R. VIETS, *Curator of the Boston Medical Library, Boston, Massachusetts.*

No one can doubt the fact that Benjamin Waterhouse was an egotistical and contentious character. He walked with an iambic gait, but the long step was far greater than the short, for he did introduce into the then known America, Jennerian "Kine-pox"—a term coined by himself—when he boldly vaccinated his son on 8 July 1800. The result was successful, and this private experiment opened the way to the establishment of the value of cowpox vaccination as a preventative of smallpox. Waterhouse must have sensed this, but the method that he used to control the supply of vaccine and restrict its diffusion in order to profit from it as a monopoly was indeed, as Blake has pointed out in his reappraisal of the man, "morally indefensible." This fostering of the spirit of commercialism, moreover, actually damaged the cause of vaccination, for most of his brother physicians of 1800 did not hold with monopolistic ideas invading an altruistic profession. When Waterhouse asked the potential vaccinators to bond themselves to remit a quarter of their profit to him if he supplied the vaccine, many turned away from these drastic demands. James Jackson, just returned to Boston from London in September 1800, set a better course, for when Benjamin Waterhouse promised him vaccine but delayed the

supplying of it, young Jackson went to Thomas Manning of Ipswich who gave him all he asked for (having received a supply from his brother in London) and refused all compensation. Jackson, Manning, and others may have made a small charge for vaccination but made no attempt to monopolize the supply.

Although Benjamin Waterhouse may have made "something handsome by it," as John Warren thought, the time was short, for by November, four months later, the vaccine matter was in the hands of all the physicians of Boston, free from Waterhouse's bonding restrictions. Waterhouse thus failed to monopolize the new discovery largely because his peers viewed his actions with suspicion and hostility. There was nothing of the saintly Zabdiel Boylston, his predecessor in smallpox prevention, about the prevaricating Benjamin Waterhouse, and yet it seems unlikely, in spite of the local furor, that anyone but Waterhouse, with his European background, could have made vaccination a national accomplishment or even visualized and put into action a nationwide program such as he succeeded in doing. In spite of the grimy material which Blake has so carefully swept up into a neat pile there still emerges the picture of a great figure in American medicine, a dynamic physician, who, always keeping an eye on the main chance, ultimately influenced the broad progress of medicine so that much of the underlying spoilage receded by Time's unremitting cleansing. Benjamin Waterhouse, even with his lack of forthrightness, takes his place in medical history as a benefactor of the human race. Blake, moreover, by etching in the dark lines, has made the picture even clearer. Meticulous in his research by turning up obscure and forgotten bits of factual material, particularly the story of the ill-fated bonds and the feeble attempt made by Benjamin Waterhouse to defend himself against the rightful charge of commercialism, Blake has written a creditable book, an excellent example of medical historical scholarship at its highest standard.

ANTOINE LAURENT LAVOISIER. *Oeuvres,* Tome VII. Correspondence receuillie et annotée par René Fric. Fascicule II, 1770-1775. Paris, Editions Albin Michel, 1957. pp. 251-536.

Reviewed by DENIS I. DUVEEN, *New York City.*

IT is highly gratifying to see publication of the great scientist's correspondence proceeding regularly and to welcome the second fascicle. This volume, for each fascicle is in bulk the equivalent of an average size book, contains slightly more than 150 letters and covers the years 1770-1775.

The year 1770 covered a period when Lavoisier was journeying in the provinces on behalf of the tax-gathering *Ferme Générale* and almost all the correspondence for this year was addressed to his superior in that organization, Jacques Paulze, who was to become his father-in-law a year later. One cannot help being impressed by the young man's balanced judgment and the good sense shown in his handling of the numerous controversial subjects that had to be decided. A good insight into the organisation

and working of the *Ferme* can be obtained by a study of his letters and reports.

The remaining years embraced in the present fascicle include correspondence with many well-known scientists and give a clear picture of the ever-widening sphere of Lavoisier's interests and connections. A highly valuable feature of the publication is provided by the copious and very detailed footnotes which give biographical and bibliographical details concerning the characters that flit across the stage. These are packed with information and materially add to the value of the work as a reference tool.

The year 1774 is the date of appearance in print of Lavoisier's first major publication, *Opuscules Physiques et Chymiques.* He decided to give his ideas the widest possible circulation by sending copies of the book to numerous academies and scientific societies as well as leading scientists at home and abroad. The drafts of the covering letter employed in many of these cases are to be found in the present fascicle. The great care taken to ensure that each letter fitted the particular recipient is a good illustration of Lavoisier's painstaking attention to detail that he was consistently to show in all his endeavours.

A letter written from London by Magellan to Madame Lavoisier (December 1775) impresses one with the degree to which the eighteen-year-old girl had evidently associated herself with her husband's work and interests. The same year a bill from Baumé (page 533) for a cough medicine for Lavoisier's aunt introduces one of the few human touches to be found. The striking lack of the human touch raises the doubt as to whether at some stage the correspondence was not gone through by Lavoisier's descendants with the object of removing any documents of a personal nature.

The letters throw much light on contemporary French, and English, science and thought. As the years go by they become increasingly interesting. The complete *Correspondance* will obviously be an indispensable adjunct not only for students of the life of Lavoisier and the history of chemistry but also for all interested in the eighteenth century scientific and political ferment.

W. R. BETT. *Sir John Bland-Sutton. 1855-1936.* Edinburgh and London, E. & S. Livingstone, Ltd.; Baltimore, The Williams & Wilkins Co., 1956. 100 pp. $4.75.

Reviewed by FRANK GLENN, *Lewis Atterbury Stimson Professor of Surgery, Cornell University Medical College, New York City.*

THE author, Dr. W. R. Bett, has produced an interesting and informative biography of Bland-Sutton by describing selected events in his life against a background of contemporary people and places. The story is so well done that the reader gains in a remarkably short reading time the over-all picture as well as intimate details of the life of one of England's outstanding surgeons and his times. This is efficient and effective reporting at its best.

It also whets the appetite to know more about what has not been mentioned.

Sir John Bland-Sutton was one of England's strong surgeons. As the author emphasizes, "work, always work and yet more work" was the basis for his accomplishments. From an environment of limited opportunity he rose to become a scholar, teacher, and scientist contributing to many areas in biology and medicine. A determination to know, coupled with deep interest and thinking about what was not known, equipped him to be a pioneer in several fields. His great capacity for work did not deprive him of the exercise of his well-developed sense of humor, which enabled him to adapt himself readily to people and places previously totally unfamiliar to him.

In gynecological surgery his concepts were sufficiently original to convince many that he was primarily a nonconformist. Persistent, as well as energetic and brilliant, he established his beliefs but not without often stirring up considerable rancor.

Few people plan openly to secure the highest post in their academic pursuits. However, that is just what Bland-Sutton did. The presidency of the Royal College of Surgeons was evidently selected as his goal early in his surgical career. He gained his F.R.C.S. in 1884. He repeatedly sought election to the Council, and was successful in his sixth attempt, in 1910. Eight years later he was vice president, and president from 1923 to 1926. This position he enjoyed to the full as well as the activities of a practice commensurate with the distinction. It was characteristic of the man that the positions he sought he fulfilled with distinction. He knew what he wanted and seems seldom if ever to have deviated from his objective.

If one feels that something is missing in this brief biography he will probably ask why the author has not said more about Sir Anthony Bowlby, Bt., who preceded Bland-Sutton as president of the Royal College of Surgeons, and his successor, Sir Berkeley Moynihan, Bt. This being the peak position of recognition that he sought and attained, one could well enjoy a comparison.

Brief biographies rarely are so complete. This one is not only interesting and informative but in places exciting. It is good reading for the medical student as well as those interested in surgery and English history.

THOMAS NEVILLE BONNER. *Medicine in Chicago 1850-1950: a chapter in the social and scientific development of a city.* Madison, Wis., The American History Research Center, 1957. xvii, 302 pp. Illus. $5.00.

Reviewed by JOHN B. BLAKE, *Associate Curator, Division of Medical Sciences, U. S. National Museum, Smithsonian Institution, Washington, D. C.*

FOR everyone interested in the history of medicine in the United States this is an important book. In the first place it is an excellent account of

the history of medicine in Chicago, which is in itself a desideratum. Perhaps even more important is what it represents in the way of medical historiography: a local history of medicine sponsored and supported by the local medical society, but written by a professional historian with full and free access to the society's files and complete freedom to express his own opinions and conclusions. The Chicago Medical Society deserves heartiest congratulations for this enlightened attitude, and the author for the result. *Medicine in Chicago* is written with full cognizance of concomitant social, political, and economic factors as well as the scientific, and it does *not* have a section—is this not unique in a volume so sponsored—devoted to biographies of the Society's founders and presidents. As the Committee on Medical History of the Chicago Medical Society states, "This is a new adventure in the writing of medical history." (p. xvii)

The author, who holds a Ph.D. in American history, writes with the background and from the viewpoint of a general historian. Moreover, Dr. Bonner writes as one who is unfettered by any special allegiance to the institution or group he describes. His opinions and conclusions on personalities, politics, ethics, economics, and other controversial socio-medical questions are stated with refreshing frankness. The Committee on Medical History has stated with equal frankness that it disagrees with some of the author's conclusions. "A layman," writes the Committee, "may not always appreciate the power that some esoteric factors such as the Code of Medical Ethics have on the lives of members of the medical profession." Perhaps this is because, as Dr. Bonner shows, so many of the objections to changes in the social and economic arrangements of the profession raised by medical societies on grounds of "ethics" have so obvious a relationship to the pocketbooks of private practitioners. The disinterested observer may be pardoned if he believes that the motives of physicians—like those of other mortals—are mixed; and physicians themselves may profitably read this book if they would understand how their actions appear to others. One of the many merits of this volume is that it shows how often the conflicts and problems of the profession revolve around questions with a direct economic bearing, a fact often ignored in older and more conventional local medical histories. As Dr. Bonner writes, the Chicago Medical Society "became aware of its power [through the Code of Ethics] to combat economic heresy: free clinics, contract medical schemes, and voluntary health insurance plans." (p. 210) It is hardly surprising if laymen, who are not bound by the professional Code of Ethics, see nothing "unethical" (in the common sense of the term) about such efforts to make more widely available the benefits of medical science.

Medicine in Chicago, however, covers much more than economic matters. The author has described the development of the city's hospitals, medical schools, societies, and journals; the impact of scientific advances in medicine; health and disease in their social setting; public health administration; and leading medical personalities. (One is struck by the

pervasive role of Nathan Smith Davis: his name, according to the index, appears on 45 of the book's 223 pages of text.) Dr. Bonner has related the medical history of Chicago not only to its social setting locally, but also to the medical history of the nation as a whole. (I noted with regret two misspelled names of non-Chicagoans: Marion "Simms" for Sims [p. 74] and "Samuel" for Lemuel Shattuck [p. 183].) The book has the usual scholarly apparatus—notes, bibliography and index—is well documented, well written, and attractively produced. It is a valuable contribution to the history of medicine, not only of Chicago, but of the nation as a whole. As Richard H. Shryock writes in the Foreword, "it is to be hoped that we shall have future, comparable histories of other cities and also of some of the states. Studies of this nature would be of value to all social historians, and would meanwhile provide the elements essential to any general history of medicine in the United States." According to the dust-jacket blurb, Dr. Bonner is now Director of the Medical History Project sponsored jointly by the University of Kansas and the Kansas Medical Society. This reviewer, for one, looks forward to reading the book that he trusts will result.

BRYAN GANDEVIA. *An Annotated Bibliography of the History of Medicine in Australia.* With a foreword by Sir Gordon Gordon-Taylor. Monographs of the Federal Council of the British Medical Association in Australia, No. 1. Glebe, Sydney, Australia, Australasian Medical Publishing Company Ltd., 1955. 140 pp. £1/15/—.

Reviewed by WILLIAM C. GIBSON, *Kinsmen Professor of Neurological Research, University of British Columbia, Victoria, Canada.*

THIS is a most fascinating little volume containing approximately eight hundred items, selected and annotated by the scholarly Honorary Curator of the Medical History Museum of the Medical Society of the state of Victoria, located in Melbourne. It contains both author and subject indexes.

To read through this clear and discerning bibliography in an evening is to come to grips with the solid literary background of Australian medicine, to understand the hardships of the pioneers both lay and medical, and to appreciate more fully that blending of British and South Seas medical interest. Perhaps the relative geographic isolation of Australia in the past has made for a greater interest in local medical history than is found in other continents. That a high proportion of the medical profession do take an interest in Australian medical history is evidenced by the large number of memorial orations which have been set up to honor the worthy pioneers in medical science in a vast and underpopulated part of the world. Not only are the orations memorials to medical pioneers, they often deal with medical history. They have been preserved for us by that truly great, if unheralded scholar, the late Mervyn Archdall, the

seemingly ageless editor of the *Medical Journal of Australia* who died on 6 September 1957 in his sleep. Australia's medical historical literature is rich in texture, thanks largely to the wisdom and unceasing struggle for scholarship by Dr. Archdall. To see him with his long grey locks hanging over his perspiring forehead in the publishing house in Sydney, surrounded by papers, books, editorial assistants, and printing machinery, is something not soon forgotten.

Dr. Gandevia was able to complete his labor of love with a grant from the Wellcome Foundation and the Victorian Branch of the British Medical Association in Australia. His work is that of a real scholar, penetrating, literate in many fields including those of exploration, botany, and oceanography. Thus the bibliography leads us through the medical aspects of the discovery and exploration of Australia, the medical aspects of the England-Australia transportation, to the medicine of colonial times, the evolution of modern medicine in Australia, the specialties, tropical diseases as they affected Australia, preventive medicine and child welfare, hospitals, medical societies, medical education, aboriginal medicine, nursing, journalism, and biography.

The great explorers of the world had in many cases Australian chapters in their lives. Some of the places named in and about Australia recall not merely the great explorers but sometimes the medical personnel attached to voyages of exploration and collection. The currents of reform which swept through British medicine and nursing, such as those set afoot by Florence Nightingale, are mirrored in the medical historical literature of the Southern Continent.

One is conscious also of the novels and historical books by Australian physicians. A keen sociological interest has marked out some of these. Military considerations have played a very large role in the lives of several generations of Australian medical men. The numbers lost in the First World War were simply staggering. In the Second World War the numbers imprisoned for very long periods were great. One has only to visit the national war memorial in Canberra to realize the war effort made by this thinly populated country.

Dr. Gandevia has done a very great service to medical historians everywhere because until now there has been no real key to the rich medical history of his continent. Australians themselves will benefit immeasurably by this little volume's publication, for medicine in the various states of Australia is too often isolated and requires some federating influence to bind it together as a whole. The more recent developments in the provision of medical care, hospital care, and pharmaceuticals throughout the country has made it necessary for Australian medicine to know its own history as never before. Thus, Dr. Gandevia's annotated bibliography fulfils many useful functions in bringing to his countrymen their own history, bringing it to the notice of all medical libraries wherever situated and, best of all, bringing together the wonderful story of innovation and adapta-

tion by resourceful Australian physicians over two centuries in a land replete with early problems and now great promise.

REGINALD C. MCCRANE. *The Cincinnati Doctor's Forum.* Cincinnati, The Academy of Medicine of Cincinnati, 1957. 389 pp.

Reviewed by LINDEN F. EDWARDS, *Chairman, Department of Anatomy, The Ohio State University, Columbus, Ohio.*

CINCINNATIANS take great pride in the fact that the "Queen City of the West" has long been recognized as an important medical center; the city has been the nidus of a number of medical colleges and it offers superior clinical advantages as a result of its extensive system of hospitals. Moreover, its Academy of Medicine has unquestionably added lustre to the city in the field of medicine.

This volume is an account of the founding and progress of the Academy of Medicine of Cincinnati, which was organized on 5 March 1857. It is, therefore, a history of the first one hundred years of that organization and was published during the present year by the Academy. The author, Dr. Reginald C. McGrane, is Professor of History in the University of Cincinnati.

The work is not merely a documented account of the activities and internal events of the Academy throughout its century of existence, it is in fact a history of the medical profession in Cincinnati. It includes biographical sketches of many of the city's leading practitioners and the roles they played in the educational, political, civic, and professional life of Cincinnati, as well as in various historical events, such as the Civil, Spanish American, and both World Wars.

Of special interest is the story of how the Academy crusaded energetically for such local reforms as purification of the water supply, sanitary dairy regulations, immunization against diphtheria, vaccination against smallpox, and improvement of hygienic conditions in the public schools. Of national import was the Academy's vigorous sponsorship of the organization of the American Association of Medical Milk Commissions (3 June 1907), of a Smoke Abatement League, of the National Tuberculosis Exhibit in the city (1906), and of national legislation to advance the interests of the medical profession.

The chapter headings give a good indication of the subject matter covered and include such topics as Cincinnati and the Medical Profession in 1857, Founding of the Academy of Medicine of Cincinnati, The Academy of Medicine of Cincinnati during the Civil War, Progress and Dissension, Reunion and the New Spirit, The Academy 1900-1920, The New Era, Another World War and its Aftermath, and the Academy in Historical Perspective. The last chapter contains a roster of Academy members who made notable contributions to organized medicine, to the various medical

and surgical specialties, to medical journalism, and to the health and public welfare of their community. A list of the officers and members of the Academy, 1857-1957, is given in the appendix. Indicative of the extensive historical research done by the author in the preparation of this fascinating and illuminating account of the Academy of Medicine of Cincinnati is the large bibliography. The index is quite adequate, but unfortunately there is a paucity of illustrations.

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